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Three Essays on Migration, Migration Policies, and Migrants' Integration

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*Standing there on the embankment,
staring into the current, I realized
that—in spite of all the risks
involved—a thing in motion will
always be better than a thing at rest;
that change will always be a nobler
thing than permanence; that that
which is static will degenerate and
decay, turn to ash, while that which
is in motion is able to last for all
eternity.*

Olga Tokarczuk, *Flights*

Introduction

If migrations have always characterized human history, in recent decades the size of migratory phenomena has boomed to an unprecedented size. Only in Europe more than 23 million individuals have a non-European citizenship, and around 2.3% of Europeans live outside their home countries (European Commission, 2021). While the freedom of movement and the internationalization of educational and working careers gave a boost to the mobility of young and high-skilled individuals, conflicts and climate changes have increased the number of refugees and asylum seekers. While some countries experience large migrant inflows, other countries lose thousands of citizens to the search for better social, political economic conditions.

While some governments worry about restricting immigration, integrating incoming immigrants in their labor markets and societies, predicting the effects that migrant inflows might have on natives' employment and political attitudes, other governments worry about containing emigration, recalling emigrants, predicting the effects that migrant outflows might have on the economy and society. No matter which perspective is taken, whether migrants themselves are considered, or natives at destination, or stayers at origin, migratory phenomena entail challenges and opportunities for both destination and origin countries. For this reason, it is fundamental to deeply understand the determinants of migration, the effects that migration has on migrants themselves, on natives and stayers, and more in general on countries. At the same time it is crucial to seek evidence of how governments and supranational organizations may intervene to better integrate immigrants in their labor market and societies, to re-attract emigrants or benefit from the network of nationals abroad, to attract new immigrants and welcome successfully refugees and climate migrants.

In the last decades studies on migration in the social sciences have grown exponentially. Many of these are the starting point of my dissertation. In Chapter 1, I revise the theoretical and empirical literature on migration economics, focusing

on three aspects that form the basis for my empirical analysis. First, I focus on the determinants of migration, on the understanding of which are the personal motives, which are the conditions for individuals to decide in favor or against migration, which are the contextual drivers that push or pull them into migrating. Second, I review studies on the consequences of migration for the migrants, answering what happens to their labor market outcomes, their integration into host societies, and which factors improve or hinder their integration. I also review which are the consequences of migration more broadly, for countries, which is eventually the drivers to implement policies that attract and integrate immigrants, but that also might ban and discriminate them. The last part of the chapter is dedicated to reviewing previous literature on these policies.

Drawing from this vast literature, my empirical chapters have three shared themes: the evaluation of policies for the integration and attraction of immigrants, the focus on high-skilled migrants, the focus on the decision to remigrate to the home country. They also have in common the institutional setting, Germany. Germany represents an ideal country to study migration and in particular migrants in the labor market for two reasons. First, it has been experiencing large migration flows since the early 60s. Going from the Guestworkers programs to the recent refugee crisis, Germany has welcomed groups of immigrants from all over the world who have now grown into large diaspora communities and have contributed to the creation of a multicultural society. Second, Germany collects and provides access to incredibly rich and useful data to answer a wide variety of questions on the integration of migrants in society and the labor market.

In Chapter 2, I evaluate the effect of a reform that strongly facilitated the possibility to recognize professional certificates acquired abroad independently from the country of origin. Since EU migrants already benefited from a standard European framework, the reform especially targeted non-EU migrants for whom before 2012 no legal basis was available for the recognition of certificates. In this chapter, I investigate whether the reform successfully increased recognition rates for non-EU immigrants, but also whether it allowed non-EU migrants to enter regulated

occupations. Additionally, I analyze whether Germany traded-off the quality of workers with recognized certificates to improve immigrants' integration.

In Chapter 3, I focus on the brain drain phenomenon and evaluate a policy response of the Italian government. Specifically, I evaluate a law that introduced large tax deductions for high-skilled Italians returning to Italy after 2010. Three eligibility criteria had to be fulfilled upon return: being born after 1969, having a university degree, having resided abroad for at least 2 years. I take two perspectives, using both Italian and German data, and investigate whether the law was successful in re-attracting high-skilled Italian emigrants to Italy. Using detailed German data, I also analyze to what extent the law changed the selection of returning Italians in terms of observable and unobservable skills. From a policy perspective, understanding which emigrants tax deductions are able to attract back is important for the policy cost-benefit calculations.

In Chapter 4, I expand the focus and consider the integration processes of all immigrants independently from their educational level or country of origin. Specifically, I consider the decision to return and to integrate in the host labor market, and analyze one specific factor largely understudied. That is, socio-political conditions in the home country. Combining survey data, social security records and precise information on terrorist attacks (which I use as proxies for socio-political shocks at home), I ask whether these violent events in the home country affect migrants' return intentions and labor market behavior (e.g. time in unemployment).

Overall, my dissertation explores different aspects of migratory phenomena. The chapters build on and aim to extend the existing literature along several dimensions, especially with respect to the mobility and integration of high-skilled migrants, the evaluation of migration policies and the understanding of the link between home country conditions, return intentions and migrants' integration effort. Nonetheless, the findings in the dissertation aim also to inform policy makers on the effects (and potential side-effects) of migration and integration policies, both to improve on existing policies and to better orient the design of future ones.

Chapter 1

A Brief Review of the Theoretical and Empirical Literature on Migrations, Migrants' Integration and Migration Policies

Abstract

This chapter reviews pioneering and recent literature on the economics of migration with a particular focus on high-skilled migration. High-skilled migration is a growing phenomenon that has recently attracted large attention both in the media and in academia. Many countries face constant challenges to attract and retain high-skilled migrants, both to increase their human capital and to mitigate the negative effects of brain drain. In this chapter I first review the current trends in high-skilled migration, highlighting its main characteristics such as the rapid increase in the last decades, the main geographical directions, and its circularity. and it Second, I review studies that investigate both the determinants and the consequences of high-skilled migration, considering both the individual and the country perspective. Third, I review studies that evaluate different types of policies aimed at attracting and retaining migrants and. I focus on policies that aim at integrating migrants in the host labor market, such as the recognition of credential and naturalization policies. Finally, I discuss policy implications of these studies for the further implementation and improvement of migration policies.

1 Introduction

With the advent of globalization and the interconnection between countries and labor markets, migration flows have evolved in their density, roots and characteristics. According to the most recent Eurostat data, in 2020 around 3.3% of EU citizens live and work in a EU country different from their country of origin ([European Commission, 2021](#)). 2.7 million individuals entered the EU from non-EU countries, adding up to the more than 20 million individuals who live in Europe but have a non-EU citizenship ([European Commission, 2021](#)). Since 2015 Europe has also been the destination of a large number of refugees from Middle-Eastern countries, fleeing from conflicts and natural disasters. From these broad statistics, it appears immediately clear that migrations are a key phenomenon in European countries, and an a crucial topic in their policy agenda.

Migration phenomena are far from being static, and in the last decades they

changed both in magnitude and type. From the years of mass migration, through the labor migration of the post-war years until the recent mobility, migration flows have strongly changed. If at the beginning of the previous century entire families of Europeans were moving to distant destination seeking fortune and chasing the American dream ([Abramitzky et al., 2012](#)), in the 60s and 70s it was mainly men, low-educated workers, from Southern European countries going for predefined periods as “guestworkers” in the growing continental economies ([Martin and Miller, 1980](#)). The 80s and 90s were characterized by family reunifications and refugee flows from the ex-Yugoslavia and – more rarely – from the dismantled Soviet Bloc ([Barsbai et al., 2017](#); [Dustmann et al., 2017](#)).

Today migratory phenomena are again rapidly changing. For example, in recent years migrants are younger and more highly educated. This trend has been favored by the expansion of higher education, together with its internationalization (e.g. Erasmus program and similar), the sinking of transportation costs, the possibility to access freely labor markets, and the need for specialized labor in many destination countries (e.g. health care specialists and engineers). Migration flows are also more temporary and circular. Individuals do not only move multiple times between destination and home countries, but also move across destination countries. Finally, in recent years many countries have experienced an increase in the arrival of refugees and asylum seekers. Conflicts in Africa and Middle East, as well as numerous natural disasters and climate shocks have brought many people to leave their countries and seek shelter in European countries.

All these changes have raised a number of challenges for origin and destination countries, from the distribution of refugees during the 2015 Refugee Crisis, to the selection and integration of immigrants in the labor market, to the effects that migratory phenomena have on natives’ attitudes, labor market prospects and political behaviors, to contrasting brain drain phenomena. Migration is a fundamental aspect of societies as it not only affects the life and career of mobile subjects, but it also has broader implications for the singular country internal dynamics (sending or receiving) and for international equilibria. These implications, moreover, are surely economic,

but also social and political, as the growth in populist and xenophobic ideas and movements has demonstrated. It is therefore necessary to adopt multiple lenses and multifaceted points of view in order to investigate the numerous aspects of migration and their evolution over time.

For this reason, together with migrations themselves, also the study of migrations has become increasingly diffuse, and many disciplines are contributing to study different aspects of migration. In recent years researchers from demography, to whom the topic of migration traditionally belongs, but also from economics, sociology and other social sciences have been publishing a prolific number of works. Sub-disciplines such as economics of migration (Borjas et al., 2019) and sociology of migration have expanded both the number of approaches, techniques, and the scope of the topics investigated. In this introductory chapter to the empirical analyses, I review the literature on migration, restricting the focus on two general aspects. First, I mainly - although not exclusively - focus on high-skilled migration. Second, I give importance to the policy aspect of migration, reviewing the literature on policies that attract and integrate immigrants.

The chapter is organized in four parts. I first describe in more detail the phenomenon of high-skilled migration, especially in the OECD and European context. In a second step, I review the literature on the determinants of migration, distinguishing between studies that focus on how individuals decide whether to migrate and where to migrate, and studies that investigate how contextual factors may work as both push and pull factors to migration. In this section, I also consider the decision to remigrate to the home country. In a third step, I review studies that investigate the consequences of migration for individuals. I refer to the consequences of migration as the process of socio-economic integration in the host-country. In a fourth step, I focus on migration and integration policies, as institutional factors that may affect both the decision to migrate and the consequences of migration for migrants, but also natives and stayers at origin. Finally, I conclude the chapter discussing how the empirical work in the dissertation relates to the literature I reviewed in the previous sections.

This chapter aims to be an introduction to the empirical analysis. For this reason, it is not meant to be a complete review of the literature in the economics and sociology of migration, nor a specific review on a few key topics. Instead, its goal is to broadly present different aspects of the migration literature in which my empirical chapters are embedded.

1.1 High-skilled migration

In recent years the number of high-skilled individuals moving from one country to another has been increasing, both in absolute terms and relative to the lower educated groups. From 1990 to 2010, the migration of high-skilled migrants increased by 130%, and reached 28 million mobile individuals worldwide. This increase did not just involve migration flows from low-income countries to high-income countries. Also across developed countries the movements of high-skilled individuals have grown and evolved. In Europe, for example, about 2.3% of highly educated active EU-28/EFTA citizens have been living in an EU or EFTA country other than their country of origin for up to ten years ([European Commission, 2021](#)). This rise in high-skilled migration did not only occur in absolute terms, but also relative to lower-educated immigrants ([Docquier and Rapoport, 2009](#); [Arslan et al., 2015](#)). Figure 1 plots countries based on the share of high-skilled emigrants and high-skilled population. In most countries, the share of high-skilled emigrants is higher than the share of high-skilled individuals in the whole population. Focusing on Europe, the share of high-skilled amongst mobile EU citizens grew from 23.7% in 2010 to 30.9% in 2020 ([European Commission, 2021](#)).

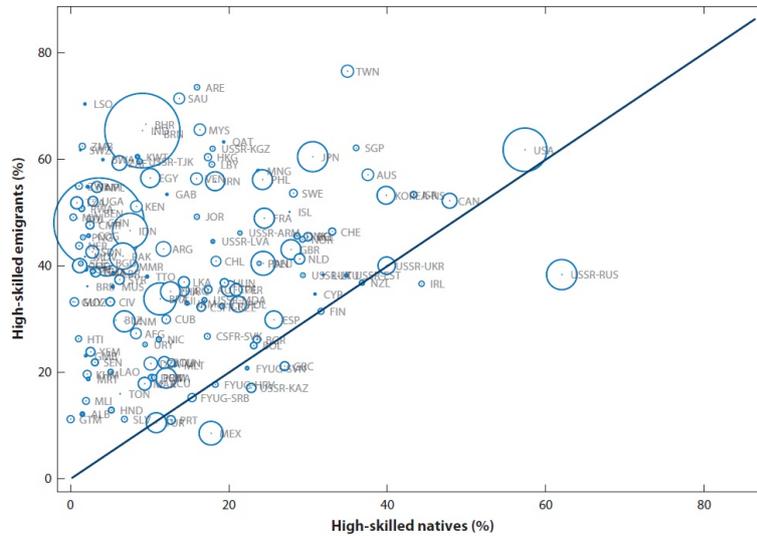
Recent efforts to collect and standardize data on migration have allowed to improve the characterization and the study of high-skilled migration. Datasets such as DIOC-E from the OECD, Eurostat Migration flow statistics and open data from census statistics have been widely used to understand the evolution of both stocks and flows. Among the emerging patterns, one in particular stands out: while the number of origin countries increases, the number of destination countries tends to

be constant and fairly small. Concentration phenomena of high-skilled immigrants can be observed in few destination countries, especially the USA, Canada, Australia. At the European level there is a strong agglomeration of high-skilled migrants in the UK, Germany and the Scandinavian countries (Fries-Tersch et al., 2018). More in general, OECD countries host two-thirds of the worldwide pool of high-skilled migrants OECD (2016). Moreover, based on the occupation, sector or industry they work in, high-skilled migrant workers tend to cluster in single areas of a country. An example is the large group of Asian scientists and high-tech experts who moved to the Silicon Valley, a phenomenon which provides first-hand evidences on the fact that concentration phenomena are even stronger in the upper tail of the talent distribution (Borjas and Doran, 2012).

Concentrated migration patterns are also typical of the migration of university students (also defined as raw talent migration) who decide to spend an entire cycle of studies in a foreign country. Also in the contest of student mobility agglomeration is even clearer for the top performers who, for example, move often to the American Ivy Leagues or England's best colleges. Given the magnitude of inflows and outflows, countries can be then classified as either net importer, net exporter or as displaying a balance between incoming and outgoing high-skilled migrants. For example, the UK and Germany for Europe, and Australia, Canada and the USA on the global stage are net importer. European net exporters are, among others, Lithuania, Romania (Fries-Tersch et al., 2018).

Besides this agglomeration phenomenon, other recent trends have been detected. Above all, the notable increase in the migration of high-skilled women (Docquier et al., 2009), who between 1990 to 2010 increased by 152%, from 5.7 to 14.4 million (Kerr et al., 2017), and the rise in shorter-term and circular migrations (Kerr et al., 2017; Dustmann and Görlach, 2016a).

Figure 1: High-skilled emigrants and high-skilled population in OECD countries



Notes: Figure 1 reports the share of high-skilled population (natives) on the x axis, and the share of high-skilled emigrants relative to the total of emigrants. Source: Calculations by [Kerr et al. \(2017\)](#) on DIOC-E data

2 The Decision to Migrate and Return

Why, Where, When At any point in time, individuals may decide to migrate for the first time, to move to a new country if they are already abroad, or to return back to their own countries. Since the seminal works by [Sjaastad \(1962\)](#), the economic literature has conceptualized these decisions as cost-benefit calculations. Individuals weight the costs and returns to migration, both monetary and non-monetary, and decide in favor of migrating if the benefits are higher than the costs. Examples of monetary returns are the present and discounted future returns to migrate in terms of wages, career opportunities. Examples of non-monetary returns are higher standards of living, higher satisfaction with life and career, the possibility of offering a better future to family members abroad or at origin. Examples of costs are travel and housing costs, which may be higher at destination, but also adaptation costs such as the need to learn a new language or to apply for VISA or to create a new network. Importantly, perceived costs and returns should be also considered, as they enter the decision-making process and vary across social background, individual networks and accumulated life experiences ([McKenzie et al., 2013](#)).

Research studying the decision to migrate has focused on three big questions: 1) who migrates, who selects into migration; 2) where and when individuals migrate, how they choose the destination and the optimal time of migration or return migration; 3) why individuals with some characteristics and from some countries of origin move to a particular country or areas of a country. Both questions 2 and 3 focus on the sorting process of migrants. Focusing on high-skilled migrants, in terms of selection and sorting, the economic literature agrees upon the fact that highly educated: tend to be more mobile in every origin country both during the educational and the occupational career; tend to move further away (Kerr et al., 2017); they are more likely to move to countries in which wage inequality is higher (Parey et al., 2017). These trends may be explained in terms of costs and benefits of migration. In order to move, high-skilled migrants must expect a wage premium (together with other non-monetary returns) following migration. In case they are mobile workers, they therefore choose destination countries in which there is a skill-premium on the skills they have acquired through education at origin; in case of mobile students the same reasoning applies, but the decision to migrate can be also driven by the wage-premium in the origin country related to migration on its own or to the gains from particular skills/networks acquired abroad in educational systems of a higher quality.

The highly educated are likely to face also lower adaptation costs as their educational level should have provided them on average with stronger relational and learning skills. Additionally, since migration is often subject to barriers (for example statutory restrictions on the number of legal immigrants) and individuals must compete to “access” migration opportunities, high-skilled migrants are likely to perceive relatively lower costs in terms of time and effort (invested in the pre-migration process). Finally, they are themselves a selected group with respect to social background, so that the weight they put on perceived returns to migration is higher than the perceived migration costs. All in all, if migration is considered as an investment, the high-skilled will be in general more represented in the migrants’ pool, because of higher expected and returns and lower perceived and actual costs, and will sort themselves in destination countries in which their investment is maximized.

The large amount of work testing the Roy/Borjas model provides some indirect empirical evidence about earnings differentials and labor market performance of immigrants. In general, most results suggest that immigrants do not earn more than natives simply due to mobility. The observed wage premium tends to be rather explained by individual skills: the high-skilled move to countries in which their ability is rewarded with better economic outcomes on the labor market. This is true both in the migration from developing to developed countries – many works analyze migrations from Mexico or Puerto Rico to the United States (see for example, [Moraga \(2011\)](#); [Patt et al. \(2021\)](#) –, and in the case of mobility across developed countries. Results hold also considering only the pool of high-skilled migrants. For example, [Parey et al. \(2017\)](#) tested the Borjas model for German graduates moving within or outside Europe. For both types of mobility, they find that the high-skilled among the graduates self-select into countries where they can earn more (ex, USA in case of outside Europe mobility, Switzerland in case of within Europe mobility). [Assirelli et al. \(2019\)](#) show for the case of Italy that high-skilled migrants (university graduates) tend to earn higher wages and have better employment probabilities than native peers with similar observable characteristics, a fact that speaks in favor of self-selection.

A related strand of literature - important for the empirical analysis - investigated a decision that necessarily follows the initial decision to migrate. That is, whether migration is temporary. As discussed in the introduction to this chapter, temporariness is a typical feature of recent migrations. From an individual perspective, research on the economics of temporary migration has focused both theoretically and empirically not only on the decision to return to the home country or move on to a new country, but also on the optimal timing of this decision (that is, the length of stay in the host country). These studies have modelled the decision to return under different individual preferences ([Adda et al., 2016](#); [Dustmann and Görlach, 2016b](#)) and shown that this decision and its timing has important consequences for human capital accumulation, consumption, remittances, and more broadly the integration of immigrants abroad ([Dustmann and Mestres, 2010](#); [Dustmann, 2003, 1999](#)).

Push and pull contextual factors. The literature on migration decisions, briefly revised in the previous section, tends to consider migration as a stand-alone individual decision. However, benefits and costs vary greatly with the context migrants are embedded in, and may vary across time and space as well. While in Section 4 I mostly focus on policies that may act as push or pull factors, here I review more broadly the literature on push and pull factors, intended as contextual characteristics that affect migration decisions. Push factors are contextual characteristics which push individuals to emigrate from their home country or from the host-country in the case of migration movements towards a different country or back to the origin country. Several works have analyzed the effects of push factors on migration intentions and realizations. Examples of such factors are bad economic conditions and economic crises ([Anelli and Peri, 2017](#); [Anelli et al., 2020](#)), violent and unexpected events such as wars ([Bahar et al., 2022](#)), natural disasters and weather conditions ([Dustmann et al., 2017](#); [Harari and Ferrara, 2018](#)), forced migration as in the case of Polish deportees in the Kresi region after WWII ([Becker et al., 2020](#)).

All these factors originate in the home countries and act on individuals by forcing them into migration or change the perceptions of costs and benefits connected to migration. However, push factors may also originate in the host country and push migrants into migration or return migration. One recent example is xenophobic violence ([Steinhardt, 2018](#)). On the other hand, pull factors are characteristics at destination, such as demand shocks for specific occupations/nationalities ([Abarcar and Theoharides, 2020](#)) changes in migration policies, the Eastern Enlargement ([Giesing and Laurentsyeva, 2017](#)), as well as the introduction of HB1 Visa in the US ([Mayda et al., 2018](#)), the size of the ethnic network at destination ([Anelli et al., 2020](#)), change in the attractiveness of one destination relative to others ([Bertoli and Moraga, 2013](#)). Again, pull factors may also originate in the home country and attract back migrants (Malaysia case and Yugoslavia case of return migration).

Finally, two points are worth noticing. First, push and pull factors do not necessarily act on their own. For example, [Anelli et al. \(2020\)](#) study the emigration

of young, high-skilled Italians from 2008 onwards and show that the economic crisis worked as a push factor, but at the same time the network of Italians abroad worked as a pull factor. Therefore, Italians moved from provinces hit more by the crisis and towards locations with larger networks of Italians abroad. Another example is bilateral agreements between countries, which work as both pull and push factors increasing migration from the origin to the destination country with which the agreement has been established (Czaika and Parsons, 2017). Second, at a more individual-level dimension, also the family may act as a push/pull factor: in fact, dynamics within the family play a role in changing individual's migration intentions (see Murard (Murard) for a critical review of migrants' self-selection within households).

3 Integration of immigrants in the labor market

The literature on the consequences of migration for individuals is vast. Migration may not only have consequences for individual migrants, but also for natives (see Dustmann et al. (2016) for a review), for stayers in the home country (see for example Dustmann et al. (2015); Elsner (2013)), and for families left behind (for example, Antman (2010)). Since my empirical chapters focus on the labor market integration of immigrants, in this section I summarize the literature on the economic consequences of migration for the individual migrant. For migrants themselves, the act of migrating may have a large impact on several aspects: from labor market prospects, to family formation decisions, to changes in beliefs and attitudes. From an economic perspective the literature has characterized the process of integration in various terms: wages, probability of remigrating, risk of overeducation, future employment prospects. From a more socio-demographic perspective, studies have shown that migration may have an impact, among others, on the acquisition of fertility and marriage norms of the host country (Gathmann and Monscheuer, 2020), or of natives' attitudes (Jaschke et al., 2021).

The very reason why researchers have begun to look into selection and sorting processes (see previous section) is the observational fact that (high-skilled) migrants

perform relatively well in the labor market of destination (Borjas, 1987). In the United States the observed age-earnings profiles are steeper than those of natives and they cross them after a period of adaptation. Still, in many contexts the assimilation of migrants is low and gaps between natives and migrants, as well as migrants from different countries of origin are present. Numerous works have shown that in Western countries the economic assimilation of migrants has remained slow or has even slowed down in the last decades (Borjas, 2015). Persistent wage and unemployment gaps between migrants and natives have been observed in a large variety of contexts (Algan et al., 2010).

An extensive literature has investigated the relationship between individual characteristics and economic assimilation. Host-country language proficiency (Ferrer et al., 2006; Dustmann and Van Soest, 2002; Bleakley and Chin, 2004, 2010; Yao and van Ours, 2015), the age at arrival in the host country (Bleakley and Chin, 2010; Friedberg, 1992), the participation in ethnic enclaves (Xie and Gough, 2011; Battisti et al., 2016) should all affect both the initial entrance in the host country labor market and the length of the assimilation process. Moreover, as previously mentioned, return plans and remittances may also affect the integration of immigrants (Dustmann and Mestres, 2010; Dustmann, 2003).

Restricting again the attention to high-skilled migrants, one crucial factor for their economic success in the host country is the transferability of human capital acquired at home (or in previous migration spells) to the host country. While this topic is relatively understudied, a limited number of works has shown for different contexts that imported skills may have smaller returns compared to skills acquired in the host country (see Dustmann and Glitz (2011) for an overview). These lower returns have been found both in terms of wages (see, for example, Friedberg (2000) and Bratsberg et al. (2002)), the likelihood of being over- or under-educated (Chiswick and Miller, 2009) and the probability of receiving a job offer (Bratsberg et al., 2002). Transferability of foreign-acquired human capital is therefore less-than-perfect in many migration contexts. In other words, part of the human capital that migrants acquired in the home country before migration is lost or underutilized in the post-migration

period and this should affect returns to foreign human capital. In Chapter 2, I focus on this topic and investigate the integration effects of a policy that aimed at improving the transferability of migrants' human capital.

4 Migration and integration policies

As a large part of my dissertation focuses on migration and integration policies, in this section I review the literature distinguishing between policies aimed at integrating immigrants and policies that aim at attracting them. Two other types of policies are not considered here but are still worth mentioning: policies that exclude immigrants from the labor market (such as employment bans for refugees, see for example ([Fasani et al., 2021](#))), and policies that foster emigration such as sponsored training programs ([Giorcelli, 2019](#)) or educational programs (e.g. Erasmus).

4.1 Migration policies

A large set of policies aim at influencing migration flows both in terms of size and composition. In this section, I provide examples and related studies to summarize different types of migration policies. Many countries nowadays use migration policies not only to regulate the size of incoming flows but also to select immigrants. Examples of these policies are the positive lists, the EU-Blue Card initiative, bilateral agreements, and the U.S. VISAs for high-skilled immigrants. [Czaika and Parsons \(2017\)](#) analyze the effects of numerous migration/selection policies worldwide and show that point-based systems are more effective in attracting high-skilled migrants, than systems requiring a job offer, labor market tests or using shortage lists. On the contrary, offers of permanent residency tend to be more attractive for low-skilled workers so that they overall reduce the human capital content of labor flows. [Mayda et al. \(2018\)](#) show that a cut in the HB1 VISAs (VISAs for high-skilled migrants) changed the pool of applicants towards immigrants with computer-based occupations and from India.

Similarly, [Abarcar and Theoharides \(2020\)](#) find that the increase in VISAs for nurses increased the immigration of nurses from the Philippines. Other policies aim at reducing the settlement costs for new migrants. Denmark, for example, successfully affected its migration inflows by increasing welfare benefits for non-EU exclusively ([Agersnap et al., 2020](#)) or by offering tax deductions to top earners ([Kleven et al., 2014](#)). Still other policies aim at reducing or preventing immigration. Examples of these policies are yearly caps on immigrants, or the temporary restrictions that most EU countries adopted to contain the inflow of Eastern Europeans after the Eastern European Enlargement. Finally, migration policies may be used to re-attract individuals back to their home countries. In this case, the target of the policy is return migrants rather than incoming migrants. These policies have been used, for example, to re-attract emigrants in case of brain drain phenomena (see for example, [Del Carpio et al. \(2016\)](#)). In Chapter 3, I evaluate of these policies that offered large tax deduction to high-skilled Italians returning to Italy.

4.2 Integration policies

While migration policies may also affect the integration of immigrants (see, for example, the case of Positive Lists or the EU Blue Card initiative), many countries have adopted public policies that specifically address the integration of immigrants into the host labor market, and more in general into the host society. Many integration policies take the shape of programs that target specific groups or needs, intervening on the determinants of integration revised in the previous section. These are for example language courses ([Arendt et al., 2020](#); [Lochmann et al., 2019](#)) and job search programs ([Joona and Nekby, 2012](#); [Sarvimäki and Hämäläinen, 2016](#); [Battisti et al., 2019](#)). These studies show that these types of interventions may have positive effects on the integration of migrants who would have otherwise lagged behind relative to natives and other groups of migrants. Nonetheless, these programs tend to be small-scale and are difficult to scale-up due to the high organizational and bureaucratic costs.

A different type of integration policies are nation-wide initiatives that affect

a large part of the immigrant population and relatively low-cost. One example are naturalization policies, that is changes in the requirements to acquire the host country citizenship. Germany represents an important case study. In 1991 and 2000 it underwent major revisions to its naturalization rules. In 1991 it introduced age-based residency requirements for citizenship: depending on the age at arrival in Germany the years of residency needed to acquire citizenship varied. In 2000, Germany switched from a *ius sanguinis* to a *ius soli* birthright citizenship criterium. Children of immigrants born in Germany after January 1st 2000 would automatically be granted the German citizenship if at least one parent had spent 8 years in Germany. Different studies have shown the importance of these reforms for the labor market integration and social integration of first-generation migrants (Gathmann and Keller, 2018; Avitabile et al., 2014), and for the educational choices and cultural assimilation of second-generation migrants (Felfe et al., 2020; Dahl et al., 2020).

Similar to naturalization policies, countries have enacted policies that issue a large quantity of resident permits to illegal migrants. These policies may also have positive effects on the integration of immigrants. Pinotti (2017), for example, show that the granting resident permits to illegal migrants reduces their criminal activity. Other large-scale policies may target other aspects of integration. In Chapter 2 of my dissertation I focus on large reform that introduced a legal basis common to all immigrants for the recognition of certifications acquired abroad.

5 Discussion

This chapter served as introduction to the empirical chapters of the dissertation. In the different sections it presents aspects of the economics of migration literature that form the basis for the empirical chapters. These mainly focus on four aspects: migration and integration policies, return migration, high-skilled migration and migrants' integration. I started from a description of recent statistics and trends in migration, in Europe especially, and focused on high-skilled migration. I then revised the literature from which the empirical chapters stem. Specifically, I summarized studies on the decision to migrate and return to the home country, focusing both

on the decision-making process of the individual migrant and the contextual factors that influence migration decision.

I then reviewed the literature on the consequences of migration, focusing on the consequences on immigrants themselves. I paid particular attention to the role of individual characteristics, such as language proficiency and transferability of human capital acquired abroad. As a last step, I analyzed the literature on policies, distinguishing between policies that aim at integrating migrants in the labor market and societies, and policies that aim to attract and select potential immigrants before their decision to actually move.

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Chapter 2

Making Integration Work?

Facilitating Access to Occupational Recognition and Immigrants' Labor Market Performance

This chapter is joint work with Silke Anger (University of Bamberg, Institute for Employment Research (IAB) and IZA) and Malte Sandner (Institute for Employment Research (IAB)). We thank Uschi Backes-Gellner, Kamila Cygan-Rehm, Christian Dustmann, Albrecht Glitz, Libertad Gonzalez, Simon Janssen, Giuseppe Ippedico, Adrian Lerche, Teresa Monteiro, Markus Nagler, Cem Özgüzel, Enrico Rettore, Jens Ruhose, Uta Schönberg, and conference and seminar participants for their insightful comments. We also thank the Federal Institute for Vocational Education and Training (BIBB) and in particular Jessica Erbe and Nadja Schmitz for data support. Any omissions or errors are the sole responsibility of the authors.

Abstract

This paper exploits a reform that facilitated the recognition of foreign occupational qualifications for non-EU immigrants but not for EU immigrants in Germany. Using detailed administrative social security and survey data in a difference-in-differences design, we find that the reform increased the application probability for the recognition of non-EU immigrants by 5 percentage points, raising the share of non-EU immigrants in regulated occupations (e.g., nurses) by 18.6% after the reform. Moreover, despite the large inflow of non-EU immigrants in regulated occupations, we find no evidence that these immigrants received lower wages or that they had lower skills.

1 Introduction

Immigrants experience worse labor market outcomes than natives in most host countries (e.g., [Borjas, 2015](#); [Algan et al., 2010](#)). A large part of this gap results from barriers to the transferability of immigrants' skills to the host country ([Hendricks and Schoellman, 2018](#)), often because home country certificates do not allow immigrants to enter regulated occupations in the host country ([Tani, 2017, 2018](#)). This under-utilization of immigrants' skills may explain the occupational downgrading that immigrants experience in many host labor markets, leading to high individual and society welfare losses (e.g., [Friedberg, 2001](#); [Mattoo and Ozden, 2008](#); [Dustmann and Preston, 2013](#)).

Recent research has shown that occupational recognition — the formal proof of the equivalence of a foreign certificate to its native counterpart — can enhance the transferability of qualifications and give immigrants access to regulated occupations, with strong positive effects on their labor market outcomes ([Brücker et al., 2021](#); [Sweetman et al., 2015](#)). However, access to recognition in most host countries is both non-standardized and costly for immigrants. For example, as no legal framework (neither national nor federal) for recognition exists ([Rabben, 2013](#)), U.S. authorities makes recognition decisions case by case. Consequently, application rates are generally low and only those who expect to gain the most on the labor market apply ([OECD, 2017](#)). Thus, in the last two decades, major destination countries have been discussing ways of restructuring recognition procedures ([ILO, 2016](#)).

This is the first paper to evaluate a unique reform that introduced a standardized framework for the recognition of professional qualifications open to all immigrants, independently of the country of origin. Specifically, we examine the Federal Recognition Act, passed by the German government in 2012, which (a) introduced a legal basis for recognition, (b) standardized and facilitated the proof of equivalence between German and non-German certificates, and (c) established numerous sources of information about recognition procedures for immigrants. Importantly, the quality standards for foreign certificates to receive recognition did not change. Given these characteristics the Federal Recognition Act presents a potential blueprint for recognition reforms in other countries.

Despite the clear goals of the reform, its effects on the labor market integration

of immigrants are a priori ambiguous. First, a facilitated recognition framework does not necessarily translate into higher recognition rates. For example, the costs of applying may still exceed the expected gains from recognition in the labor market. Alternatively, the reform may have attracted applicants who did not meet the unchanged recognition standards. Second, even if recognition rates increase, the reform might fail to integrate immigrants into the labor market. For example, the facilitated application process may have attracted immigrants who had successfully had their home country certificates recognized but who still had lower observable and unobservable skills than pre-reform. In such cases, even if their foreign certificates fulfilled all recognition criteria, the immigrants' actual or perceived skills may still not meet employers' standards in the post-reform period. Therefore, employers may not hire these immigrants or may offer them lower-quality jobs in terms of earnings, job security, or other employment conditions.

To identify the effects of the reform on immigrants' labor market integration, we exploit the fact that, since 2005, immigrants from inside the EU were subject to recognition process similar to that of non-EU immigrants after the reform. Thus the reform allowed us to apply difference-in-differences (DiD) designs in which EU immigrants represent the control and non-EU immigrants the treatment group. EU immigrants constitute a legitimate control group because they (a) must also have their home country certificates recognized to work in regulated occupations and (b) face language barriers similar to those of non-EU immigrants. Thus the DiD design rules out the possibility that better labor market outcomes for non-EU immigrants after the reform are merely the result of better economic conditions that coincide with the reform. Had economic conditions improved after the reform, both EU and non-EU immigrants' employment, wages, and out-migration should have been similarly affected.

We take advantage of detailed German survey and administrative social security data in our DiD approach. The survey data allows us to analyze the reform effect on recognition applications because it provides detailed retrospective information on the recognition applications of both EU and non-EU immigrants and on each applicant's socio-demographic and economic characteristics. The administrative social security data allows us to analyze the reform effects on both wage growth and employment

in regulated occupations. The data includes all non-German individuals in the labor force, over 13 million individuals both five years before and after the reform. Beyond many socio-economic and employment-related immigrant characteristics, the data also contains information on occupations, allowing us to identify whether an immigrant was employed in a regulated occupation before and after the reform.

We obtain four key results. First, we analyze the reform effect on non-EU immigrants probability of applying for recognition. Results show that applications increased by 5 percentage points after the introduction of the standardized recognition framework. Second, we investigate whether the positive effects on applications transfer to an increase in employment in regulated occupations. Importantly, as we are mainly interested in the integration effect of the reform (not how it influences immigrant inflow), our main analysis concentrates on immigrants who lived in Germany before the reform. We find that the employment probability in regulated occupations for EU and non-EU immigrants developed parallel during the five years before the reform and diverged sharply after it. In regulated occupations with the highest application rates (mainly health care), the probability of employment in regulated occupations for non-EU immigrants increased by 1.8 percentage points, an increase of 18.6 percent. In regulated occupations with the lowest application rates, we don't find any employment increase.

Third, we use the panel nature of the administrative data to examine whether – due to the reform – immigrants with recognized certificates but with lower observable and unobservable skills selected into regulated occupations. We show that for non-EU migrants, neither the characteristics of the last employment spell before moving to a regulated occupation nor the average earnings in these occupations changed as a results of the reform. Fourth, we investigate the reform effects for non-regulated occupations. While a recognized certificate is not mandatory for working in these occupations, the reform encouraged application for recognition, because a recognized certificate may act as a signal of quality and thus facilitate employment. We find that the employment of non-EU immigrants in these non-regulated occupations increased after the reform by 3 percentage points, an increase of 6.8 percent.

Our results are robust to a series of potential biases and identification threats. To deal with concerns about selective out-migration, we restrict our sample to individuals

working in Germany throughout 2007-2017, and use the total number of immigrants in regulated occupations as the main employment outcome. Further concerns may be related to the choice of the EU15 as our control group. Although we clearly show that trends in employment are parallel for both EU and non-EU immigrants pre-reform, factors such as discrimination may vary differently between EU and non-EU immigrants over time. Therefore, as alternative control groups, we also use (a) non-EU immigrants with education acquired in Germany and (b) German workers.

Finally, to handle additional threats from time-varying unobservables correlated with the timing of the 2012 reform, we exploit the additional state-time variation induced by the staggered adoption of regional recognition laws from 2012 to 2015. This estimation confirms the baseline results and shows that the probability of non-EU immigrants being employed in occupations regulated at the state level increases by 29 % after the introduction of regional recognition laws.

As our paper is the first to analyze the causal effects of a recognition reform, it makes several important contributions to the literature on the economic integration of immigrants. First, we contribute to the literature on policies aimed at improving immigrants' labor market integration. Numerous studies have investigated the relationship between individual characteristics and economic assimilation. These include studies of host-country language proficiency ([Ferrer et al., 2006](#); [Dustmann and Van Soest, 2002](#); [Bleakley and Chin, 2004](#)) the age of arrival in the host country ([Bleakley and Chin, 2010](#)) and residence in ethnic enclaves ([Cutler and Glaeser, 1997](#); [Xie and Gough, 2011](#); [Battisti et al., 2016](#)). Drawing on this literature, other studies have evaluated policies targeted at individual immigrants, such as language courses ([Arendt et al., 2020](#); [Lochmann et al., 2019](#)) and job search programs ([Joonas and Nekby, 2012](#); [Sarvimäki and Hämäläinen, 2016](#); [Battisti et al., 2019](#)). However, whether these policies are scalable in a cost-efficient way remains unclear.

In contrast, fewer studies have focused on nation-wide integration policies, which by definition target all or a large group of immigrants and are inexpensive to implement. One exception is [Gathmann and Keller \(2018\)](#), as they study the effect of two nation-wide citizenship reforms in Germany on immigrants' labor market integration. They find that reduced residency requirements for citizenship have

positive effects on employment and earnings, particularly for women. Our results add to the literature on national integration policies, showing that changes in recognition policies appear to be a cost-efficient way of improving labor market integration for large groups of immigrants. As standardizing recognition can be implemented in many countries, our results are highly important for policy makers worldwide.

Second, we contribute to the growing literature on occupational recognition in two distinct ways. We complement research on the labor market gains from recognizing foreign certificates. For example, [Kugler and Sauer \(2005\)](#), find that recognizing a medical degree clearly improves earnings. [Brücker et al. \(2021\)](#) and [Tani \(2017\)](#) analyze gains from recognition for a larger group of occupations and find that immigrants who acquire a recognized certificate have both higher employment probabilities and higher earnings. Our results confirm that labor market gains from recognition hold across different occupations, but additionally show that this finding also holds in a context where recognition procedures are easier and the inflow of immigrants into regulated occupations is larger. Moreover, in contrast to these other studies, we demonstrate that recognition also affects employment in non-regulated occupations.

We also contribute to research on how changing requirements for working in regulated occupations affects the selection of workers in these jobs. For licensing requirements – the requirements of natives to work in regulated occupations – studies have demonstrated that changing requirements affect the quality of service and workers ([Shapiro, 1986](#); [Anderson et al., 2020](#); [Larsen et al., 2020](#)). In our setting, easier access to recognition may have effects comparable to lowering licensing standards leading to a lower average productivity and lower wages of applicants. We show that easier access to recognition can improve the integration of immigrants without altering the average productivity of the applicants. These results are highly policy-relevant, as other countries may also face a trade-off between integrating high-skilled immigrants and maintaining high-quality workforce standards.

Third, our results contribute to research understanding of how immigrants make decisions about having their home countries' certificates recognized in host countries. Our results indicate that bureaucratic hurdles and uncertainty about the outcome constitute an important obstacle to immigrants' applying for recognition. As the

returns to recognition appear high, this finding is surprising. However, it is in line with those studies showing for other groups (e.g., students and welfare recipients) that small changes in application procedures can strongly increase take-up ([Bettinger et al., 2012](#); [Hoxby and Turner, 2015](#); [Bhargava and Manoli, 2015](#)). Our results show for the first time that immigrants are also very sensitive to application procedures. Policy makers need to take this decision process into account when they make decisions on recognition frameworks.

The rest of the paper is organized as follows. Section II describes the institutional setting in which the empirical analysis takes place. Sections III and IV describe the empirical framework and the data, while Section V presents the main results. Sections VI and VII provide additional analyzes. Section VII concludes.

2 Institutional Background and Potential Mechanisms

2.1 Institutional Setting and Recognition of Foreign Certificates

Working in a regulated occupation in Germany requires a domestic professional qualification or, for immigrants, the formal recognition of their foreign qualification. [Brücker et al. \(2021\)](#) calculate that regulated occupations make up around 12 percent of total employment in Germany, of which are in the health sector (e.g., physicians, pharmacists, nurses), 28 percent in the public sector (e.g., police officers, teachers, social workers), and 25 percent in the technical sector (e.g., architects, engineers, physicists). Occupations can be regulated at the federal level (Bundesebene) or at the state level (Landesebene) (in the private sector, 84 occupations are regulated at the federal and 111 at the state level ([BMBF, 2017](#))). Other than the responsible authority for the recognition process, these two groups of occupations hardly differ in their recognition procedures.

In contrast, entering a non-regulated occupation requires no formal recognition. Nonetheless, for most of these occupations, immigrants can apply for an official assessment of their home country occupational qualifications. If recognition succeeds, that assessment becomes a legally binding document validating the equivalence with the German qualifications. Examples of such unregulated occupations are those

requiring training (e.g., office management clerks, electricians) and advanced training occupations (e.g., master craftsperson qualifications, certified financial advisors) (see also [Brücker et al., 2021](#)).

Despite the large number of eligible immigrants and the potential gains of recognition, applying for recognition in Germany before 2012 was an unstructured lengthy process for immigrants with degrees from non-EU countries. Applicants had to face different authorities responsible for the recognition procedure and the duration of the process was unclear. Additionally, applicants had no access to financial support to cover the administrative fee, which ranged from 100 to 600 euros (120 to 720 US-Dollars) depending on the occupation and the federal state in which the application was submitted ([BMBF, 2017](#)). Thus only 20 to 30 percent of eligible non-EU immigrants applied for recognition of their home-country certificates, compared to 30 percent of eligible EU immigrants (German Microcensus, 2008), for whom the recognition procedure was easier and more structured also before 2012 (see guidelines of the European Directive 2005/36/EC).

Data from the SOEP migration sample reveal the reasons immigrants gave for not applying for recognition before 2012, despite their eligibility (Table [A.1](#) in the Appendix). Twenty-four percent of non-EU immigrants but only fourteen percent of EU immigrants said that they did not apply due to administrative constraints. Furthermore, twenty percent of eligible non-EU immigrants without application stated that they saw no chance of obtaining recognition, compared to only fifteen percent of eligible EU-immigrants. Overall, these numbers indicate that reducing administrative hurdles and increasing information may increase application rates.

2.2 The Federal Recognition Act in 2012

To reduce bureaucratic hurdles and facilitate the process of occupational recognition for immigrants with a non-EU certificate, the German parliament passed the Federal Recognition Act (*Anerkennungsgesetz*) in April 2012 on the recognition of certificates acquired abroad. The law harmonized the process between EU and non-EU certificates through three major changes for non-EU certificates: First, and most important, the new law created a legal basis for occupational recognition for all immigrants,

independent of their country of origin, and introduced the possibility of applying from abroad.

Second, the new law restructured, standardized, and facilitated procedures for accessing the equivalence between foreign and German certificates. Specifically, the new framework (1) allowed immigrants to send a standardized application form to well-defined administrative bodies, (2) allowed that the proof of equivalence considered not only certificates but also work experience in the home-country, (3) gave all administrative bodies a guideline for decision-making within three months of the application date.² Third, with the new law the government established numerous sources of information about the recognition procedure (e.g., multi-language dedicated websites, mobile apps, hotlines), sources that could be accessed both in Germany and abroad.³ Fourth, after the reform, the government offered and advertised subsidies covering the costs of the application process.

While all legal changes apply to professional and vocational qualifications and to university degrees with a clear link to regulated occupations (e.g. physicians, dentists, pharmacists), they do not apply to recognizing higher education qualifications that do not lead to a specific occupation (e.g. mathematician, chemist, economist). Nor does the new framework include the academic recognition of high-school diplomas. For occupations regulated at the state level (e.g., teachers, youth social workers, engineers, architects) each federal state passed its own Federal State Recognition Laws, between 2012 and 2014, which all adhere to the Federal Recognition Act.

Whereas before 2012 the German statistical offices barely kept records on the recognition process, with the 2012 Recognition Act German authorities began structured collection of application data including applicants' socio-demographic characteristics. These records show that since the implementation of the Recognition Act 2012, the number of applicants has steadily increased from 15,000 submissions, up to more than 60,000 per year (see Figure A.2 in the Appendix).⁴ Occupations

²After applying, immigrants may receive three types of standardized decisions: fully recognized (the only way for accessing a regulated occupation) partially recognized, and not recognized. For partial or non-recognition, applicants receive compensative measures to help them to reach full recognition.

³Appendix Figure A.1 gives some examples from the website *www.anerkennung-in-deutschland.de*, the main web portal for immigrants interested in acquiring information on the recognition procedure.

⁴The number of total applications rose to 420,000 by 2020, according to a recent report from

Table 1: Occupations with the highest application number, by type of occupation

Regulated occupations			Non regulated occupations	
Occupation	Level of regulation	%	Occupation	%
Nurse	National	23.68%	Electronics technician	12.67%
Doctor	National	22.97%	Office clerk	6.79%
Teacher	State	12.07%	Caregiver	3.89%
Engineer	State	4.58%	Trainer in office work	3.73%
Social pedagogist	State	4.29%	Commercial clerk	3.23%
Social worker	State	4.15%	Mechatronic technician	2.81%
Children pedagogist	National	4.07%	Machines mechanic	2.57%
Physiotherapist	National	3.02%	Office electrician	2.24%
Pharmacist	National	3.02%	Industrial electrician	1.82%
Educator	State	2.57%	IT-specialist	1.74%
Architect	State	2.35%	Sales clerk	1.66%
Dentist	National	2.09%	Metal technician	1.57%
Children nurse	National	1.36%	Cook	1.49%
Ostetric	National	1.50%	Heating technician	1.32%
Nurse assistant	State	1.30%	Hairdresser	1.24%
Total		93.02%		48.76%

Notes: Table 1 the most frequent occupations applied for. To identify the list of occupations we manually collected data from the regional statistical offices and selected the 15 occupations with the highest number of applications in 12 federal states (data is incomplete for Hamburg, Saarland, Schleswig-Holstein, and Bavaria), distinguishing between regulated and non-regulated occupations. For regulated occupations we report whether the regulation is federal or state. For all occupations we report the percentage of applications as total applications. The percentages are computed for the state *Hessen* for which we obtained the number of applications separately by occupations (5 digit Kldb2010 classification). Since not all occupation have applications in all years from 2012 to 2018, we took from the *Hessen* list the highest application number across all years for each occupation and computed the total accordingly. Alternative calculations (e.g., the sum of all application across all years) do not change the results.

Source: Regional Statistical Offices.

regulated at the federal level received the largest number of applications, followed by non-regulated occupations and occupations regulated at the state level.

The number of applications for specific occupations strongly varies within regulated and non-regulated occupations. Table 1 illustrates this variation, presenting the occupations with the largest total number of applications after 2012 for both types of occupations. For regulated occupations, the top 15 occupations received about 93% of all applications. Before the reform, 73% of all immigrants in regulated occupations worked in these top-15 occupations (Own calculations, based on the IEB).

Despite no causal evaluation of the 2012 German recognition reform, the German Ministry of Interior Affairs celebrated the reform as a great success (BMBF, 2017) – an assessment based on the increase of applications after 2012. However, the question of whether this application increase arose from the reform itself or from other factors (e.g., an economic boom or other policy changes), or whether the increase continues a process which had started before 2012, remains unanswered. Moreover, assuming that

the BIBB (BMBF,2020).

the reform caused the increase in recognized certifications, the effects on immigrants' labor market integration have not been analyzed yet.

2.3 Potential Mechanisms

Each immigrant with a professional home-country certificate makes a choice about applying for recognition or not, depending on the (expected) costs and benefits. In addition to the direct application costs and the document translation fees, various non-monetary costs arise. These include time investments to gather information and to interact with the administrative bodies (i.e. opportunity costs), as well as effort and potential psychological costs involved in organizing the paper work. Monetary and non-monetary costs of applying are determined by the institutional framework and may depend on individual characteristics of immigrants, such as language proficiency, personal skills, and immigrant networks.

The benefits of applying for recognition stem from better labor market prospects as immigrants with recognized certificates can enter regulated occupations and access unregulated occupations more easily, thus increasing their job quality in terms of wages and working conditions. The institutional framework determines these benefits by affecting the duration and the success probability of the recognition process, while labor market conditions influence benefits through employment prospects in the respective occupation. Moreover, benefits may vary by individual characteristics, such as the quality of the home-country certificate affecting individual success probability, and the remaining time in the labor market until retirement or re-migration.

Facilitating the recognition procedure is an intervention directly affecting these choices. Immigrants face lower monetary and non-monetary costs of the recognition process and may therefore be more likely to apply.⁵ The low application rate among eligible immigrants before the reform reveals the huge potential from increasing the number of applications. As a consequence, more immigrants whose home-country certificate meets the professional standards may have recognized degrees,⁶ and thus better employment opportunities, higher wages, and better working conditions.

⁵While we are the first analyzing the effects of easier access to recognition on immigrants' decision to apply, [Gathmann and Keller \(2018\)](#) show that facilitating the access to citizenship makes immigrants' more likely to naturalize.

⁶About 90% of all applications were successful pre-reform.

However, the potential benefits of applying for recognition may not be realized for two reasons. First, compositional changes in the pool of applicants may worsen the (perceived) quality of recognized occupational certificates.⁷ Although the reform did not affect recognition requirements, a facilitated application may have attracted immigrants with lower observables and unobservable skills than pre-reform to apply.⁸ A lower average quality of immigrants with recognized certificates may prevent employers from hiring immigrants despite their recognized occupations or from employing them in high-quality jobs at higher wages.

Second, equilibrium effects in the labor market may lower the value of recognized certificates, as a higher number of recognitions increases the labor supply in the respective regulated or unrelated occupations. In case of excess labor supply, more recognitions do not lead to additional employment or higher wages of immigrants. These equilibrium effects on immigrants' labor market outcomes will also depend on the elasticity of substitution between native and immigrant workers, as well as between different immigrant groups.

Thus far, whether the new recognition framework had the intended effect on immigrants' labor market integration is unknown. In light of the potential mechanisms, three scenarios of an overall reform effect are plausible: immigrants' labor market outcomes may improve, not change, or even worsen due to compositional changes and general equilibrium effects. Therefore, empirical evidence is needed whether facilitating access to recognition present an effective tool to improve immigrants' labor market integration.

3 Empirical Strategy

3.1 Recognition and Employment

To investigate the effect of the Federal Recognition Act on the integration of non-EU immigrants, we exploit the 2012 recognition law is eliminating the recognition

⁷These compositional changes may occur both among immigrants already in Germany pre-reform and among immigrants who migrate to Germany post-reform.

⁸The literature on licensing shows that changing licensing requirements may affect the quality of licensed workers (Shapiro, 1986; Anderson et al., 2020; Larsen et al., 2020).

process differences between EU and non-EU immigrants.⁹ Specifically, the reform introduced a formal recognition framework applying equally to all immigrants, regardless of their country of origin. While the new framework clearly improved non-EU immigrants possibility of obtaining recognition, it made no change for EU immigrants, who had enjoyed a standardized recognition process since 2005 (European Directive EC/2005/36). This variation forms the basis of our DiD design, in which non-EU immigrants are treated and EU immigrants are the control group.¹⁰

In our main analyzes, we estimate the following empirical model:

$$y_{it} = \alpha + \gamma NonEU_i + \lambda Post_t + \beta(NonEU_i * Post_t) + \epsilon_{it} \quad (1)$$

where the dependent variable y_{it} is an indicator for whether an immigrant i applied for recognition or whether the immigrant works in a regulated occupation in quarter t . We define our main outcome for the employment analysis as being employed in regulated occupations relative to any other status, including unemployment.¹¹ $NonEU_i$ is an indicator for whether the migrant's nationality is from a non-EU country, $Post_t$ is a time indicator that assigns value 1 to observations in quarters t after the new recognition law and 0 to observations in quarters before it. The parameter of interest β measures the effect of the reform on outcome y_{it} for non-EU immigrants.

In further specifications, we include a large set of controls. Individual controls include age and age at arrival (proxied by age at entry in the social security register for wage and employment models), nationality, which is our indicator for country of origin, and sex. To control for time-constant geographical trends and time-varying

⁹Agersnap et al. (2020) use a similar design to study the effect of a welfare reform applying only to non-EU immigrants while leaving untouched the welfare benefits for EU immigrants.

¹⁰To improve the validity of the control group, we exclude immigrants from countries that entered the EU during the last two enlargements. After the 2004 Eastern Enlargement, EU15 countries were allowed to apply transitional restrictions to the free movement of the new EU workers. Germany lifted these restrictions in 2011 for the 2004 Eastern countries. This event might therefore confound the effects of the reform for the group of EU13.

¹¹We choose the relative employment in regulated occupations, not the absolute employment, as we want to account for the increase in the immigrant population during the selected time window (2007-2017). In a series of robustness checks in Section 5.2, we test whether our results are sensitive to alternative definitions of the denominator and to using the log number of immigrants employed in regulated occupations. In Appendix Table A.4, we exclude unemployed immigrants from the sample, showing that the coefficient of interest remains positive and significant.

trends, we include local labor market fixed effects and year fixed effects in our full specifications. To take into account the underlying panel structure of the data, we cluster standard errors at the individual level.

Although we do not directly observe recognition procedures in the social security data, we provide suggestive evidence of the link between recognition and employment. First, we show that the effects on recognition and employment are quantitatively similar. Second, we show that the effects on employment occur only in the regulated occupations that received most of the applications for recognition (about 90%).¹²

Our identification strategy relies on the assumption that the outcomes for EU and non-EU immigrants would have followed the same trends after the reform had the standardized recognition framework not been established. While we clearly show that trends in the probability of being employed in regulated occupations were parallel in the pre-reform period, we now explain how we handle potential violations of this assumption.

First, the composition of incoming immigrants and immigrants who leave the country in the post-reform period might have changed as a consequence of changes to Germany's immigration policies. Specifically, in 2012 a law called "Blue-Card Act" granted non-EU immigrants with specific advanced degrees a work permit as long as German authorities recognized those degrees. The combination of the Blue-Card Act and the Recognition Act might have affected not only the integration but also the selection of immigrants coming to Germany¹³. Moreover, even without changes in migration policies, non-EU immigrants with specific skills may have found coming to Germany more attractive as recognition became easier. Finally, in 2015 Germany experienced a large inflow of refugees during the refugee crisis, dramatically changing the composition of non-EU immigrants.

To disentangle the integration effect of the reform from selective migration, we exclude from our main analysis those EU and non-EU immigrants who arrived

¹²Importantly, in the pre-reform period around 30% of non-EU immigrants in regulated occupations were working in occupations that received few applications, so that the distribution of applications does not correspond to the distribution of non-EU immigrants across regulated occupations.

¹³For example, in a recent paper [Abarcar and Theoharides \(2020\)](#) show that the expansion and contraction of U.S. visas for nurses in the 2000s changed accordingly both the number of foreign-trained nurses in the U.S. and the enrollment rates in nursing programs in the Philippines.

six months pre-reform or later. Applying this sample restriction, we include only immigrants who entered the German labor market more than six months before the recognition act. In Section 5.3, we expand our analysis and explore the employment effects on immigrants who arrived post-reform.

Nonetheless, even if we restrict our sample to immigrants who arrived pre-reform, our estimates might still be affected by selective in- and out-migration or sample attrition (e.g., due to self-employment periods, which our administrative data does not cover) in both the pre- and post-reform periods.¹⁴ We therefore also run our main estimation on a balanced panel of EU and non-EU immigrants observed in Germany throughout the period 2007-2017. Moreover, we show descriptively that, in terms of fields of study, the composition is stable across the same period for both groups.

A second concern is the choice of the control group. If employment probabilities for EU immigrants also changed as a result of either the reform or factors coinciding with it, the estimated reform effects would depend on the choice of EU immigrants as the control group. Employment probabilities could have changed, for example, if the skills of EU immigrants changed over time and employers started to replace EU immigrants with non-EU ones. To handle this concern, we define two alternative control groups, German citizens and non-EU immigrants with education acquired in Germany. As the reform affects neither of the groups directly, both groups are less likely than EU immigrants to be replaced by non-EU immigrants with a home country education.¹⁵ Using non-EU citizens with German education as a control group has the additional advantage that if employers' attitudes towards certain nationalities change, the attitudes should change for those with German or foreign certificates.

¹⁴On one hand, the reform might have affected immigrants' decision to leave Germany. On the other hand, economic shocks (e.g., the Great Recession) might have differently affected the labor market opportunities of non-EU and EU immigrants, which had changed the selection towards and out of Germany before 2012. Similar concerns would apply if non-EU immigrants knew about the reform before and selected into migration to Germany based on the perceived probability of recognizing their certificates. In Figure Appendix Figure A.3, we provide evidence from Google Trend data that Google searches about recognition sharply increased only around the first month of the introduction of the Recognition Act, while having remained stable in the previous months.

¹⁵For example, Signorelli (2020) shows that a selective immigration policy in France, aimed at increasing the hiring of non-EU immigrants in specific occupations, did not affect natives' employment. She explains this finding in terms of an imperfect degree of substitution between natives and non-EU immigrants.

Third, the reform might have coincided with a skill shortage in regulated occupations, so that the demand for non-EU immigrant workers increased even without changes in the demand for the other two groups. While such a change would not constitute a threat to our identification strategy, it might limit the generalizability of our results to other settings. To rule out this concern, we show that our estimates remain similar across labor markets with different levels of pre-reform labor demand in regulated occupations and different sizes of the non-EU network (i.e., immigrants with a similar background in a region) employed in regulated occupations. Moreover, we identify a group of occupations composed of non-regulated, skill-intensive occupations that faced a skill shortage in the years before and after the reform. These are the non-regulated occupations on the list of occupations suffering from skill shortage (*Mangelberufe*). We show that the effects for non-EU immigrants for these occupations (for which recognition is not mandatory) are zero.

Finally, to exclude the possibility that any additional confounder coinciding with the timing of the reform affects our results, we exploit the additional regional-quarter variation given by the staggered implementation of state recognition laws for occupations regulated at the state (Land) level.¹⁶ We show that pre-reform trends in employment are parallel, while they diverge from the first quarter after the introduction of state laws.

3.2 Selection Into Regulated Occupations

As a second step of the main analysis, we investigate whether the reform affected the sorting of immigrants into regulated occupations. Unless immigrants move to a regulated occupation in their first or last spell, the longitudinal dimension of the social security data allows us to observe them before and after they move to a regulated occupation. We take two complementary perspectives. First, we analyze

¹⁶We estimate the following model:

$$Pr[E_{it}] = \alpha + \gamma NonEU_i + \sum_{t=-P}^T \eta Time_t + \sum_{t=-P}^T \beta(NonEU_i * Time_t) + \eta_t + \mu_s \epsilon_{ist} \quad (2)$$

where Time are period dummies from 15 quarters before the introduction of the regional law and up to 15 quarters after (baseline is the quarter right before the law), η_t are year fixed effects and μ_s are federal state fixed effects. In Figure A.5 we display the timing of implementation of regional laws.

the selection into regulated occupations in terms of the previous employment or unemployment spell. We do so because, if the recognition reform increases the pool of immigrants who can formally enter regulated occupations, the unobserved quality of certificates may be lower. Therefore, non-EU immigrants who enter a regulated occupation post-reform may be more likely to move from occupations with lower earnings, with less complex tasks, or from unemployment spells. To test whether such selection occurs, we estimate Equation 1 on the characteristics of pre-transition occupational spells for the subsample of immigrants moving to regulated occupations.

Second, we investigate whether non-EU immigrants who move to a regulated occupation after the reform earn lower full-time wages compared with immigrants who made the transition before the reform. Lower average earnings could occur if, for example, employers are less able to discern between high- and low-quality certificates, and if their actual quality is lower. We again estimate Equation 1 using log hourly wage as the outcome variable and restricting the sample to full-time employees in regulated occupations. To control for changes in the sorting across regulated occupations, we include three-digit occupation fixed effects, together with the full set of controls used in all other specifications.

4 Data and Sample Characteristics

To conduct our analyses, we use two data sets. Our main data source is the German social security data, which we use to analyze the effects of the reform on the probability of being employed in a regulated occupation, compared to all other occupations, and to analyze the effect on wages for employees who switch to regulated occupations. We complement this data with detailed survey data from the IAB-SOEP Migration Sample (Brücker et al., 2014) on immigrants' application processes. This section describes the data sources and samples in detail.

Social Security Records

Our main analysis relies on the social security records *Integrated Employment Biographies* (IEB) for a random draw of 15% of the full population of immigrants

in the German labor market.¹⁷ The Institute of Employment Research (IAB) of the German Federal Employment Agency provides the data.¹⁸ The dataset includes detailed daily administrative longitudinal information on nationality, occupation, educational background, industry, employment status, and earnings records of all individuals subject to social security in Germany.¹⁹ The detailed information on the occupational groups (Classification: Kldb1988) allows us to link occupations to the number of applications in each occupation from administrative data on recognition procedures. The large number of individuals in the data also allows us to include very fine-grained controls, such as the local labor market, nationality, and the 3-digit occupational group.

We use the nationality information in the data to identify EU and non-EU immigrants. We want to minimize the possibility of including individuals who acquired a German education, and we want to exclude all immigrants whose highest acquired education is not eligible for recognition (immigrants without either vocational training or tertiary education). Therefore, we restrict our sample to immigrants with non-German nationality whose first recorded educational level was either vocational training or tertiary education and who entered the register when they were older than 23 years (if the first recorded educational level is vocational training) and older than 25 (if their first recorded educational level was tertiary education). We exclude immigrants older than 55.²⁰ In Appendix B we further explain the sample selection and the construction of nationality, education, and occupation variables. As we build quarterly cross-sections from 2007 to 2017, we exclude the few observations available for 2018 and all immigrants who exited the register before 2007. Our main sample thus includes 76,889 individuals with over 2,000,000 observations.

Table 2 presents socio-demographic characteristics of our main analysis sample

¹⁷Given the smaller sample size when we consider only immigrants who move to regulated occupations, in Section 6 we use a random draw of 70%, the maximum allowed given the size of the resulting extraction and data protection requirements.

¹⁸For the description of a 2% random sample from the IEB, the *Sample of Integrated labor Market Biographies* (SIAB), see (Antoni et al., 2019).

¹⁹For our employment analysis, we consider all immigrants in the register, both employed and unemployed. For our wage analysis, we follow the literature, e.g. (Card et al., 2013), and consider only full-time employees who report more than ten Euros in daily wages. We compute hourly wages by dividing the daily wage by eight (a standard full-time daily number of working hours).

²⁰We choose age 55 as the maximum age, because the compulsory retirement age in Germany for these cohorts is 65. Given this maximum age, only individuals who retire early leave the sample due to retirement.

Table 2: Socio-demographic characteristics of immigrants in Germany before the reform, 2007-2017

	(1)	(2)	(3)	(4)
	Non-EU		EU15	
	IEB	Mikrozensus	IEB	Mikrozensus
Female	0.47	0.46	0.39	0.42
Higher education	0.33	0.43	0.44	0.64
Age	42.33	42.10	42.49	41.89
Age entry	31.76	31.96	31.18	32.33
Years in the register	10.09	10.62	10.85	10.05
Northern and Continental Europe			0.66	0.68
Southern Europe			0.34	0.32
Eastern Europe and Russia	0.25	0.27		
Balkans and Turkey	0.26	0.26		
Africa	0.09	0.08		
Middle East	0.10	0.10		
Asia	0.19	0.18		
North and Central America	0.06	0.06		
South America	0.04	0.04		
Oceania and others	0.01	0.01		
Observations	1298243	14075	605985	6067

Table 2 reports basic statistics for the sample in the IEB and for a sample analogue in the German Microcensus. We pull together all Microcensus waves from 2007 to 2017 and compute variables to come as close as possible to the IEB sample selection, while improving on some of the variables that the IEB does not include. In particular, we replace age of entry in the IEB with actual age of entry in Germany and we replace the proxy for having acquired education abroad with actual information on education abroad. Moreover, the nationality variable is more precise. We consider only immigrants with reported year of entry earlier than 2011, again to simulate the sample selection in the IEB. We exclude resettled people as they are likely to be registered with a German nationality in the IEB data and also exclude them from our sample of EU15 and non-EU immigrants.

Source: Integrated Employment Biographies (IEB) and German Microcensus.

separately for EU and non-EU immigrants. Since in the administrative data we can only approximate the inclusion of individuals who acquired tertiary education and vocational training abroad, in Table 2 we also show the same socio-demographic characteristics using immigrants in German Microcensus (GMC). The GMC asks immigrant respondents both their year of migration and the year they acquired

their highest educational level. We can therefore more precisely identify immigrants who acquired their education abroad. The characteristics of immigrants in the IEB and the GMC are remarkably similar, with only the educational level being under-estimated in the IEB data. For this reason we test the robustness of our results to alternative definitions of the sample according to different versions of the educational variable.

IAB-SOEP Migration Sample

As a second data set, we exploit the IAB-SOEP Migration Sample to estimate to what extent the reform increased the application probability of recognition. The IAB-SOEP Migration Sample is a unique panel dataset constructed on a sample of immigrants interviewed in 2013, 2014, 2015, and 2016. questions about their nationality, immigration biography, year of arrival in Germany, and education obtained abroad. Crucial for our research question, for each respondent the data contains information whether the immigrant applied for recognition and, if so, the month and year of application.²¹

To maximize the sample size, we include all individuals 18 to 65 years who have a professional certificate or a higher education degree acquired abroad and who arrived in Germany for the first time between 1995 and 2014. After these restrictions, the sample consists of 1127 immigrants, including 687 who hold certificates eligible for recognition.

5 Main Results

5.1 Effects on Applications for Recognition

Using the IAB-SOEP Migration Sample, we examine the relationship between the introduction of a formal recognition framework and the decision to apply for recognition. This analysis is an important starting point for a) understanding whether reducing application costs for recognition is an effective policy for increasing immigrants' applications and b) justifying the subsequent analysis on the effect of

²¹In Appendix B, we describe in more detail the recognition variables in the IAB-SOEP Migration Sample and test their validity by comparing them with other sources.

the reform on labor market outcomes.

Figure 1 displays recognition rates by year and nationality group. Before the reform, both EU and non-EU immigrants show similar trends, but different levels, as EU immigrants have on average higher recognition rates. After the reform, the recognition rates for EU immigrants remained constant around 32%.²² In contrast, the percentage of non-EU immigrants with a recognized certificate increased significantly post-reform. Along with the graphical evidence, we estimate Equation 1, using the probability of completing a recognition process as our preferred outcome. The point estimates show a significant increase in recognition probability of 5.2 percentage points. That is an increase of 18.5 percent relative to the average recognition rate in the pre-reform period (see Appendix Table A.2, Panel B).

Given that we look at the percentage of recognized certificates, one could argue that faster administrative processes due to the reform – instead of an increase in applications – may drive the effect (i.e., the three months mandatory time window between application and decision). Therefore, we show in Appendix Figure A.4 and Table A.2 (Panel A) that the graph for application rates and the estimated reform effects on application probabilities – independent of the recognition process duration – are remarkably similar to the those obtained for completed recognition procedures.²³ As additional evidence on the direct effect of the reform on immigrants’ recognition behavior, we also show data from Google searches on recognition opportunities in Germany. To avoid including searches from abroad, we display data for searches that occurred only in Germany.²⁴ Appendix Figure A.3 clearly shows that the increase in Google searches for the word ”Anerkennung in Deutschland” (”Certificate recognition in Germany”) starts in proximity of the reform and keeps increasing thereafter.

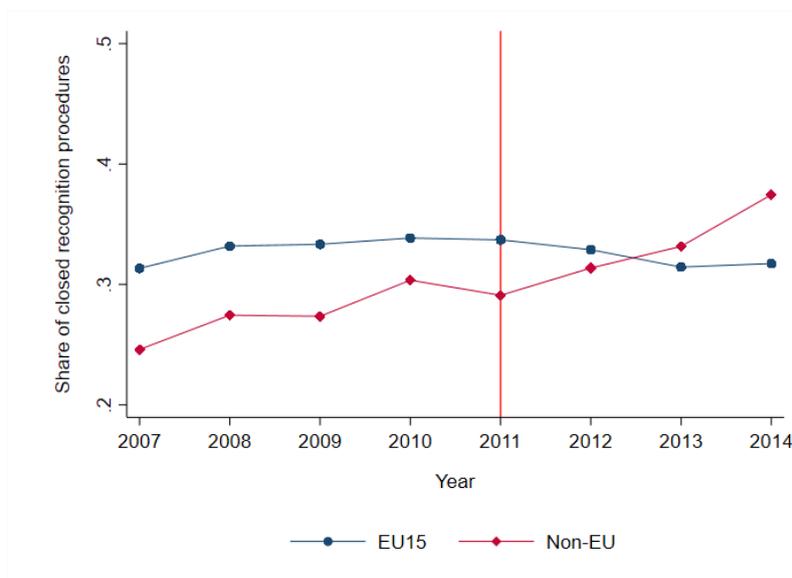
The increased rate of applications for non-EU immigrants in response to the reform constitutes an important first result for three reasons. First, it is the basis for

²²Pre-reform recognition rates computed with the IAB-SOEP Migration Sample are remarkably similar to those computed with the information for Germany from the 2008 Ad Hoc Module of the EU Labor Force Survey, see Table A.10.

²³Following our empirical strategy for the main outcomes, we also estimate the same regression models, but excluding immigrants who arrived in Germany post-reform. The results are qualitatively similar to the baseline estimation.

²⁴Google Trend data have been already shown to proxy well for individual behaviors in other contexts, such as job search (Baker and Fradkin, 2017), migration decisions (Böhme et al., 2020) and domestic violence (Anderberg et al., 2022).

Figure 1: Share of completed recognition processes by year and nationality group.



Notes: Figure 1 displays the share of EU (blue) and non-EU (red) immigrants that closed a recognition procedure in each year from 2007 through 2014. The vertical line indicates the year before the recognition reform (2011). Shares are computed combining information on the year of completion and the year of arrival in Germany. The denominator includes all eligible EU15 and non-EU immigrants who stated they were in Germany in year t .

Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016

our further analysis on labor market outcomes. Indeed, although without an effect on applications, we would not expect the reform to affect labor market outcomes, whether higher recognition rates translate into better labor market outcomes is not *a priori* clear. Second, it shows that administrative hurdles represent a high barrier to applying for recognition and that a relatively simple and inexpensive legal change had a large impact on immigrants' behavior. Third, we find virtually no change in recognition rates for EU immigrants, supporting the application of the same empirical design to labor market outcomes. In the following sections, we investigate in detail the effects of the higher recognition rate on labor market outcomes of non-EU immigrants.

5.2 Effects on Employment in Regulated Occupations

In this section we estimate Equation 1, using the probability of being employed in a regulated occupation as the dependent variable. As explained in Section 3, the sample for our main analysis includes only immigrants who appeared in the social

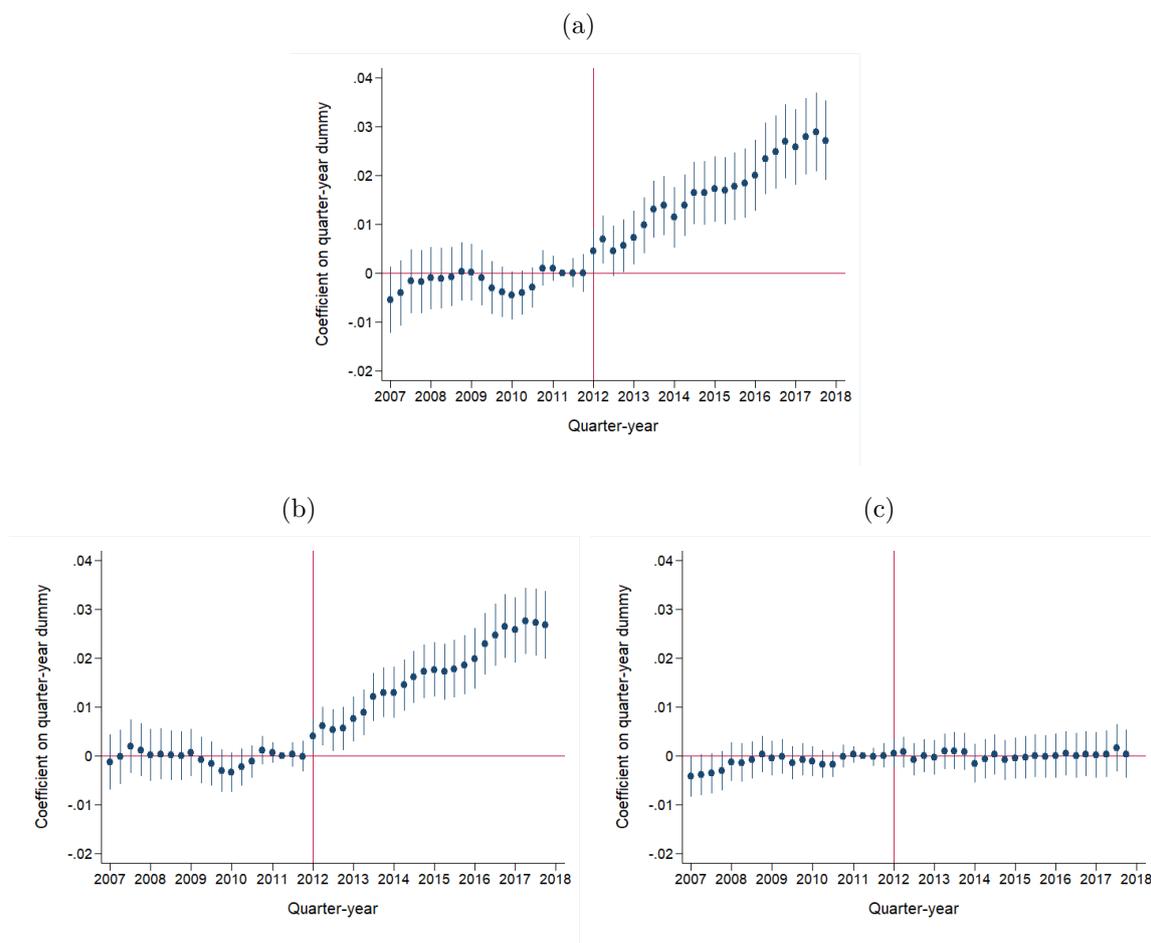
security data at least six months before the reform. The three graphs in Figure 2 display event study coefficients from the interaction between time and the nationality indicator. Graph C.1a at the top shows the differences between non-EU and EU immigrants for the probability of working in any regulated occupation. Graph C.1b, at the bottom left, plots the difference between non-EU and EU immigrants for the probability of working in regulated occupations that received the vast majority (more than 90%) of the applications for recognition. Graph C.1e, at the bottom right, plots the difference between the two immigrant groups for regulated occupations that received a low number of recognition applications (fewer than 10%). In all graphs the difference is relative to one year before the reform implementation, with the vertical red line indicating the date.

Graph C.1a clearly demonstrates a strong increase for non-EU immigrants working in regulated occupations after the reform, compared to EU immigrants. In the pre-reform period (left of the vertical line), the coefficients on the employment probability in regulated occupations did not differ significantly from zero in any quarter, validating the parallel trend assumption pre-reform. In line with our expectations, Graph C.1b shows that the post-reform increase concentrates exclusively on regulated occupations with a high number of applications. In contrast, Graph C.1e shows no effect for regulated occupations with very few immigrants requesting recognition.

Table 3 reports, for different specifications, the estimated coefficients for the interaction between nationality and the reform dummies. Column 1 shows that the probability of working in any regulated occupation increases for non-EU immigrants by 1.8 percentage points. The point estimate barely changes when the estimation includes a large set of individual control variables and group fixed effects (Column 2). The size of the coefficient appears highly important, as employment in regulated occupations increases by 18.6% compared to the baseline share (9.27%) of non-EU immigrants employed in these occupations pre-reform.

Columns 3 to 6 show the effects for the probability of being employed in regulated occupations that received the most (columns 3 and 4) or the fewest (columns 5 and 6) applications. In line with the graphs, the estimated coefficients are large and statistically significant for regulated occupations with the most applications.

Figure 2: Event study plot for the federal recognition law



Notes: Figure 2 shows the estimated coefficients and the 95% confidence intervals for the following regression model: $y_{it} = \alpha + \gamma NonEU_i + \lambda Quarter_t + \beta(NonEU_i * Quarter_t) + \epsilon_{it}$. y_{it} is the probability of being employed in (a) any regulated occupation, (b) regulated with high numbers of applications or (c) occupations with low numbers of applications, compared to any other employment state. Coefficients are estimated for each quarter pre- and post reform. The baseline is March 2011. Each coefficient represents the difference between EU15 and non-EU immigrants in percentage points from the baseline difference in outcomes.

Source: Integrated Employment Biographies (IEB).

Although the absolute effect in percentage points (1.7 pp) is smaller for occupations with the most applications than for all applications, the effect corresponds to a relative effect of 25.1 percent, which is larger than the overall effect in columns 1 and 2 combined. For regulated occupations with the fewest applications the estimated coefficients are close to zero and not statistically significant. Again, the inclusion of control variables barely changes the results.²⁵

²⁵To capture the effect of immigrants entering employment in regulated occupations both from unemployment or from a different occupation, the main specification includes both employed and unemployed immigrants. In Appendix Table A.4 we show results when the sample includes only

Table 3: Reform effects on employment in regulated occupations

	(1)	(2)	(3)	(4)	(5)	(6)
	All regulated		Regulated (many applicants)		Regulated (few applicants)	
Post*Non-EU	0.018*** [0.003]	0.017*** [0.003]	0.017*** [0.002]	0.015*** [0.002]	0.001 [0.002]	0.002 [0.002]
Baseline (Non-EU) in pp.	9.27	9.27	6.76	6.76	2.51	2.51
R-squared	0.006	0.056	0.002	0.052	0.002	0.028
Individuals	76,499	76,499	76,499	76,499	76,499	76,499
Observations	1898060	1898060	1898060	1898060	1898060	1898060
Individual Controls	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
LLM FE	No	Yes	No	Yes	No	Yes

Notes: Table 3 reports estimated coefficients and standard errors from regression models that estimate the effect of the reform on employment outcomes. The outcome variable is the probability of being employed in any regulated occupation (Columns 1 and 2), in those with high numbers of applications (Columns 3 and 4) or in those with low numbers of applications (Columns 5 and 6), compared to any other state. Each individual is assigned the value 1 if employed in one occupation in the group, and 0 if in any other labor market condition. Individuals employed, but with invalid or missing value on the occupational code are excluded. The reported baseline is the average value of the dependent variable for the treated group (i.e. Non-EU immigrants) at $t = 0$ (2007-2010). Only immigrants who were in Germany pre-reform are included in the estimation. Controls include sex, age, age squared, age at entry, age at entry squared, time in the register (and its squared transformation), nationality, educational level, year fixed effects, and local labor market fixed effects. Standard errors are clustered at the individual level.

Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies.

The size of the coefficients is highly important in absolute terms. Our 15% sample of all German immigrants includes 49,724 non-EU immigrants who were in Germany pre-reform. They represent 331,493 non-EU immigrants for the entire German population. Of these non-EU immigrants, 30,729 worked pre-reform in regulated occupations. The 18.6% increase means that, due to the reform 5,716 non-EU immigrants who worked in regulated occupation could not have done so without the reform.

In the previous section we showed, using survey data that the reform has an effect on the recognition rate of non-EU immigrants. As we do not directly observe recognition applications or outcomes in the administrative data, one might argue that the effect on employment could have also occurred without the increase in non-EU recognition rates – for example, if non-EU immigrants had obtained recognition of their certificates before the reform but only started using them after the reform was implemented.

employees and only full-time employees. The results are significant and only slightly smaller in magnitude.

However, three of our findings show the unlikelihood of this possibility. First, the effects on recognition and employment are comparable in magnitude. Second, these effects concentrate fully on occupations that received the majority of recognition applications. Third, by scaling the employment effects by the inverse of the recognition effect ($1/0.04$), we calculate back-of-the-envelope the average treatment effect of a recognized certificate on employment in regulated occupations. This effect amounts to 0.47, which is close in magnitude to the individual fixed-effects estimates in [Brücker et al. \(2021\)](#). Overall, these findings provide evidence that employment effects can be reconciled with the increase in recognized certificates.

5.2.1 Robustness Checks

As a first step we test the sensitivity of our results to alternative outcomes. As discussed in Section 3 for our preferred specification, we use a binary variable that takes the value 1 if immigrants are employment in regulated occupations and the value 0 if they are either employed in other occupations or unemployed. In Table 4, Panel A, Columns 1 and 2, we report the coefficients for regressions using either the log number of immigrants in regulated occupations or the recognition index as outcome variable. The log number measures the effect of the reform on the total number of non-EU immigrants in regulated occupations, independently from the overall number of EU and non-EU immigrants in the administrative data. The regulation index is continuous with more regulated occupations having a higher index value.²⁶ For both alternative outcomes we find a positive effect of the reform for non-EU immigrants. In Appendix Table A.8, we additionally show that the results are robust to alternative sample definitions.

We now turn to robustness tests dealing with potential concerns of our estimation strategy as outlined in Section 3. First, we test the possibility that selective in- and out-migration biases our results even after we restrict the sample to only EU15 and non-EU immigrants who arrived in Germany six months before the reform.²⁷ In

²⁶We use the continuous index including zeros for non-regulated occupations. In Appendix Figure A.6 we show the coefficient plots for different definitions of the regulation index, excluding zeros and constructing a binary variable that takes the value 1 if the regulation index is above 0.

²⁷The average length of stay in the register between 2007 and 2017 is 7 years for EU and 8 years for non-EU, allowing the possibility that our sample might be subject to changes due to selective in- and out-migration

Table 4, Panel A, we restrict our baseline sample including only immigrants who had an observation in each quarter between 2007 and 2017 (column 3) or between 2010 and 2017 (column 4), so that over the specified period our estimation samples are balanced. The results show that selective in- and out-migration in the years around the reform do not affect the effects of the Recognition Act on non-EU immigrants employment probabilities in regulated occupations. Additionally, in Appendix Figure A.7, we use the Microcensus data to identify more precisely immigrants who entered Germany between 2007 and 2017 with a certificate acquired abroad.²⁸ For both EU and non-EU immigrants, the distribution of fields of study remains almost constant throughout the time window.

Second, we test the sensitivity of our results to the choice of EU immigrants with foreign education as control group. In Panel B of Table 4 we report the results from regression models where the outcome and the treated group (non-EU immigrants) are the same as in our baseline estimations, while the control groups are either Germans with vocational or university degrees (Column 1) or non-EU immigrants who completed vocational training and higher education in Germany (Column 3).²⁹ Moreover, when we use alternative control groups, the effects of the reform for non-EU immigrants who acquired their education abroad are remarkably similar to those estimated with EU15 immigrants who acquired their education abroad as the control group. Furthermore, we show that the effects are virtually zero when we use EU15 as the treated and German as the controls (Column 2), or when using EU15 educated abroad as the treated and EU15 with a domestic education as the controls (Column 4). All results are robust to the inclusion of a large set of individual controls and group fixed effects. Overall, these results provide evidence that the choice of our control group does not drive our main results.

Third, we show that skill shortage in regulated occupations is not responsible for the effects in the baseline specification. We distinguish local labor markets by the size of the non-EU ethnic network and the extent of excess labor demand for regulated occupations in the pre-reform period.³⁰ We then run separate regressions

²⁸We do not observe this information in the administrative data, as only the educational level is collected, not the field of study.

²⁹This group is defined as non-EU immigrants who entered the register before they were 20 years old and with either vocational training or university as their highest educational level.

³⁰As explained in detail in Appendix B, to quantify demand for specific occupations we acquire

for the different groups of local labor markets. In Panel C of Table 4, we show that the estimated effects on the non-EU immigrants' probability of entering regulated occupations are similar across local labor markets with different pre-reform characteristics, ruling out the possibility that these characteristics drive our baseline results. Additionally, in Appendix Figure A.9 and Table A.7, we show that results are virtually zero for the probability of entering non-regulated, skill-intensive occupations that suffered from skill shortage in the years around the reform (part of the *Mangelberufe*).³¹

Finally, to exclude any potential unobservable confounder to the 2012 reform, we exploit the additional time variation coming from the staggered implementation of regional recognition laws that apply to occupations regulated at the state level. The timing of regional laws, summarized in Appendix Figure A.5, should be related to administrative and political processes rather than to local socio-economic conditions. Figure 3 displays the difference between EU and non-EU immigrants in the probability of being employed in occupations regulated at the state level in the 4 years around the implementation of regional laws.

While in the pre-reform period the difference between EU and non-EU immigrants is close to zero, the employment probability for non-EU immigrants increases after the passage of the state recognition law. Appendix Table A.6 (Columns 5 and 6) reports the regression results. The estimated increase is 0.7 percentage points, which corresponds to an increase of 29.0% relative to the pre-reform share of the non-EU working in occupations regulated at the state level (2.4%). These results provide additional evidence that (a) changes in the recognition legislation increase employment in regulated occupations and (b) health care occupations – which present the majority of regulated occupations at the federal level and which are subject to skill shortage – do not generate our baseline effects.

data by occupational code, year, and county (*Kreis*) on job vacancies and the unemployed. We then construct the average unemployment-to-vacancy ratio at the local labor market level, averaging across the pre-reform years (2007-2010) and regulated occupations a high number of applications. We then assign each local labor market to either high or low pre-reform demand based on whether their unemployment-to-vacancy ratio before the reform was below or above the median value across all local labor markets.

³¹The *Mangelberufe* list is a list of occupations suffering from skill shortage, similar to the positive lists introduced in other European countries.

Table 4: Robustness Checks

	(1)	(2)	(3)	(4)
Panel A	Alternative outcomes		Balanced panels	
	Log employed	Regulation index	2007-2017	2010-2017
Post*Non-EU	0.210 *** [0.046]	0.010 *** [0.002]	0.019 *** [0.003]	0.015 *** [0.002]
Baseline (Non-EU)		0.13	7.30	7.73
R-squared	0.64	0.13	0.06	0.06
Individuals		69070	20010	24490
Observations	8694	1509333	754053	683656
Panel B	Germans		Migrants with domestic education	
	Non-EU as treated	EU15 as treated	Non-EU	EU15
Post*Treated	0.014 *** [0.001]	-0,002 [0.002]	0.014 *** [0.001]	0.003 * [0.002]
Baseline (non-EU)	6.76	10.14	6.76	10.14
R-squared	0.032	0.031	0.042	0.039
Individuals	344126	312250	129139	68447
Observations	9933204	9224510	3571721	1832724
Panel C	Pre-reform demand		Pre-reform migrant network	
	<50th	>= 50th	<50th	>= 50th
Post*Non-EU	0.016 *** [0.003]	0.013 *** [0.003]	0.017 *** [0.004]	0.012 *** [0.003]
Baseline (Non-EU)	6.72	6.79	6.53	6.96
R-squared	0.053	0.056	0.072	0.043
Individuals	43658	45145	41671	48264
Observations	933882	964178	849964	1048096
Individual Controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes

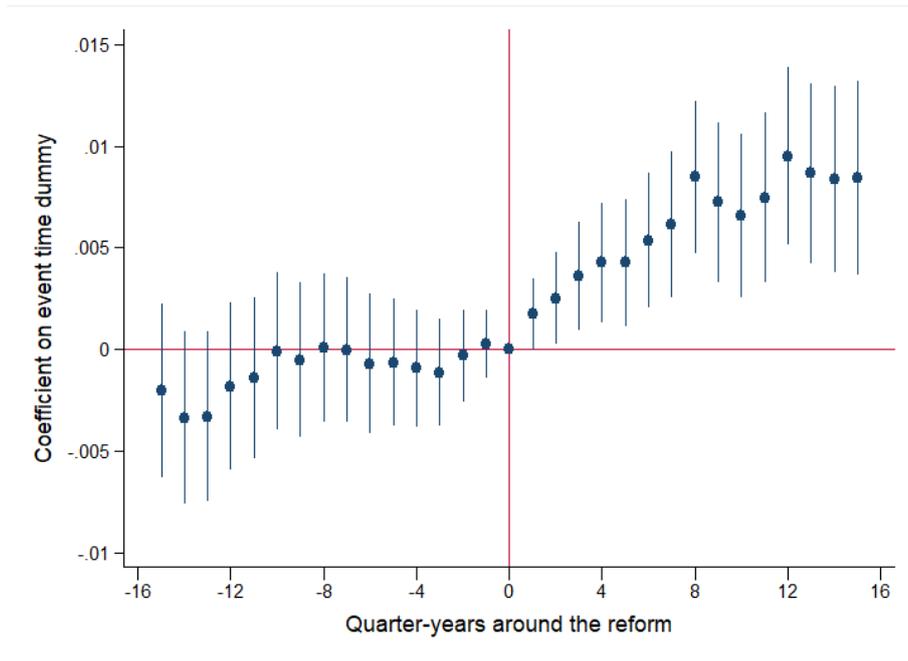
Notes: Table 4 reports estimated coefficients for a series of robustness checks. In all regressions the dependent variable is the probability of being employed in regulated occupations with high numbers of applications. In **Panel A**, Column 1 uses the log of the total number of immigrants in regulated occupations with high numbers of applications as the outcome. Column 2 uses the regulation index (Vicari 2014) as an alternative outcome. Column 3 uses the highest education level achieved instead of the first education level that appears in the register. Column 4 excludes non-regulated occupations that received high numbers of applications from the denominator. Column 5 excludes nationality with a large share of refugees (from Iran, Iraq, and Syria) from the Non-EU group. The reported baseline is the average value at $t = 0$ for the treated group (Non-EU or EU15 in the last column).

In **Panel B**, columns 1 and 2, we use as an alternative control group Germans with the highest educational level achieved through either vocational training or university. The treated are either non-EU immigrants or EU15 immigrants defined as in the baseline regressions. In column 3 we use non-EU immigrants with a domestic education. We proxy this group by including non-EU immigrants with the highest value of education (either vocational or university degree) and who entered the register before age 25. In column 4 we report results from the same regression as in column 3 but with only EU15 immigrants.

In **Panel C**, columns 1 and 2, we run separate regressions for labor markets above and below the median value of pre-reform demand for regulated occupations. In columns 3 and 4 we report the results of separate regressions for labor markets above or below the median value of the pre-reform immigrant network in regulated occupations. Controls include sex, age, age squared, age at entry, age at entry squared, time in the registered (and its squared transformation), nationality and educational level, year fixed effects and local labor market fixed effects. In the regressions with alternative control groups, we exclude the control for age at entry, because the alternative control groups are likely to be in Germany before age 25. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies (IEB).

Figure 3: Event study plot for state-level recognition laws



Notes: Figure 3 displays the coefficient from a regression model in which a time variable (-15,+15 quarters from the passing of the law) is interacted with the nationality dummy. State and year fixed effects as well as individual controls are included. The first quarter after the law passed is taken as baseline. Bars identify 95% confidence intervals.

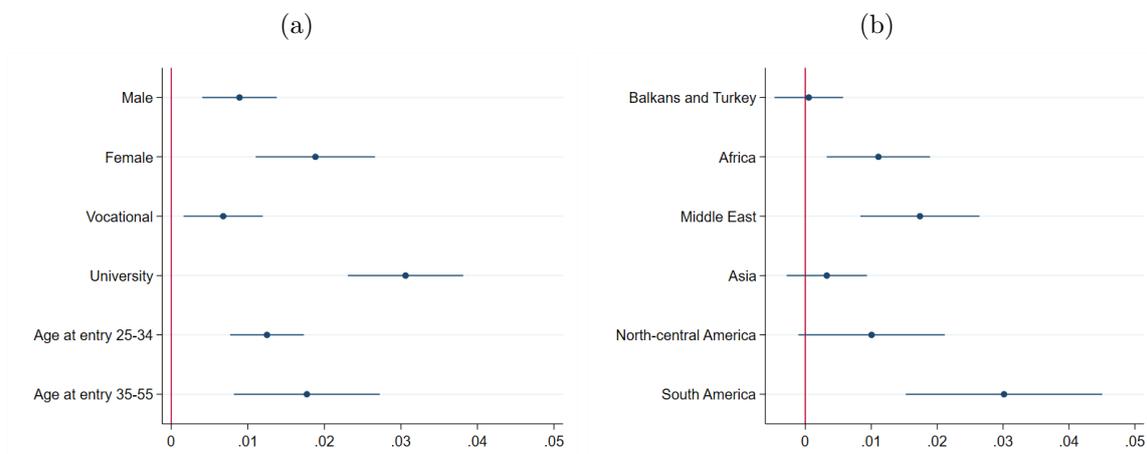
Source: Integrated Employment Biographies

5.2.2 Heterogeneity of the Effects on Employment

After showing that the effects of the reform on employment in regulated occupations are robust to several robustness checks, we now investigate how the effects on employment interact with immigrant socio-demographic characteristics and non-EU macroregions of origin. Figures 4a and 4b show the coefficients estimated from equation 1 on different subgroups of immigrants.³² The reform has larger employment effects for female immigrants, immigrants who enter employment at a younger age, and immigrants with a university degree. The characteristics of regulated occupations, in which female workers are overrepresented (e.g. nurses and social workers) and for which a university degree is often required, may explain these heterogeneous effects. For the macro-regions of origin we find that the effects are stronger for immigrants from countries in South America and the Middle East, while they are close to zero for immigrants coming from non-EU Balkan countries and Turkey as well as from those with an Asian nationality.

³²Table A.9 and Figure A.8 in Appendix show regression results and the distribution of effects over time respectively.

Figure 4: Effects of the Recognition Act for different socio-demographic groups and non-EU macroregions of origin



Notes: Figure 4 shows the estimated coefficients and 95% confidence intervals for the effect of the reform on (a) different socio-demographic groups and (b) macro-regions of origin of non-EU immigrants. Coefficients are estimated from the difference-in-differences Equation 1. The outcome variable is the probability of being employed in regulated occupations with many applications. Individual controls, year and local labor market fixed effects are the same as in the baseline estimation.

Source: Integrated Employment Biographies.

5.3 Effects on Immigrants Arrived After the Reform

This section extends our analysis to immigrants who arrived post-reform. In Table A.2 in Appendix, we showed that the reform increased application probabilities for the full sample of immigrants - including immigrants who arrived both before and after the reform. We now investigate whether the reform effect on the probability of being employed in regulated occupations also holds when we include immigrants who arrived after the reform in the employment analysis. This step is important to test whether changes in recognition laws affect not only the integration but also the selection of immigrants.

As explained in Section 3, the inclusion of immigrants who arrived after the reform may bias the reform effects for two reasons. First, the reform was enacted in combination with other migration policies that might have affected the selection of immigrants into Germany. In particular, the 2012 EU Blue Card law was intended to facilitate the entrance of non-EU immigrants in specific jobs (e.g., health care and engineering), conditional on having a job contract and a salary above a certain threshold. Second, the 2015 refugee crisis might also have changed the composition

of high-skilled non-EU immigrants residing in Germany. We examine the potential impact of both confounding factors in separate regressions, applying two different sample restrictions: (a) excluding immigrants from Syria, Iran, and Iraq, the largest refugee home countries, and (b) excluding Blue Card non-EU immigrants (and their EU15 counterparts).

Table 5, Columns 1-3, displays the coefficient from the same regressions as in Table 3 but including post-reform immigrants. As in Table 3, we find a positive effect on the probability of entering regulated occupations with the most applications. However, the effects are smaller than in Table 3 indicating that non-EU immigrants arriving in Germany post-reform benefited less than their pre-reform counterparts. Again, for occupations with the fewest applications, the effect is small and marginally significant even when including immigrants who arrived post-reform.

To investigate why the effects for regulated occupations with many applications are smaller than for the sample of immigrants who entered Germany in the pre-reform period, in Column 4 we exclude potential refugees from the sample. Excluding potential refugees (immigrants with Syrian, Iraq, or Iranian nationality) increases the effect on employment (Column 4), suggesting that this group has lower probabilities of employment in regulated occupations. Without refugees, the size of the reform effect is similar to that of the main results, indicating little post-reform selection except from refugees. Excluding non-EU immigrants who likely entered Germany through the EU Blue Card hardly changes the effect (Column 5). This finding suggests that the reform does not affect these high-skilled immigrants and confirms that the only sample selection after the reform comes from the large refugee inflow in 2015 and 2016.

6 Selection into Regulated Occupations

The previous section presented strong evidence that the reform increased the employment of non-EU immigrants in regulated occupations, all of which require recognition. Following the theoretical reasoning laid out in Section 2, in this section we test whether the reform affected the actual or perceived skill level of non-EU immigrants into regulated occupation. To do so, we first investigate whether non-EU

Table 5: Employment effects for all immigrants, including those who entered after the reform

	(1)	(2)	(3)	(4)	(5)
	Full sample			No refugees	No Blue Card
	All regulated	Regulated (many applicants)	Regulated (few applicants)	Regulated (many applicants)	
Post*Non-EU	0.014*** [0.003]	0.011*** [0.002]	0.003* [0.002]	0.016*** [0.002]	0.012*** [0.002]
Baseline (Non-EU)	9.23	6.76	2.47	6.66	6.76
R-squared	0.061	0.051	0.041	0.053	0.044
Individuals	147065	147065	147065	132553	144964
Observations	2457801	2457801	2457801	2309219	2453714
Ind. Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes

Notes: Table 5 reports the estimated coefficients from regression models with the full sample of immigrants, including those arrived both before and after the reform. The dependent variable is stated in the third row of each column. Columns 1,2,3 include the full sample, which consists of all EU15 and Non-EU immigrants independently from whether they arrived before or after the Recognition Act. In Column 4, we exclude immigrants from Syria, Iraq and Iran who arrived after 2014. In Column 5, we exclude immigrants who entered the social security data after 2012, with an employment spell in regulated occupations with many applicants, and whose hourly wage exceeded 14.95 euros (as a proxy for being EU Blue Card holder). To make treated and controls comparable we exclude both EU15 and non-EU immigrants meeting these criteria. For all regressions sample selection, individual controls and group fixed effects are the same as in the baseline regressions. Standard errors are clustered at the individual level.

Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies

immigrants who entered regulated occupations post-reform are significantly different in their labor market characteristics from those who entered the same occupations pre-reform. We then investigate whether earnings differ between non-EU immigrants working in regulated occupations pre- and post-reform.

We start by showing graphically in Figure 5 the distribution of only non-EU immigrants along the earnings rank distribution (Figure 5, left) and the regulation index (Figure 5, right) pre-reform (dashed lines) and post-reform (solid lines).³³ The blue lines in both panels show the density in occupations that non-EU immigrants held before moving to regulated occupations. The red lines show the density in occupations for the same non-EU individuals after they moved to regulated occupations.

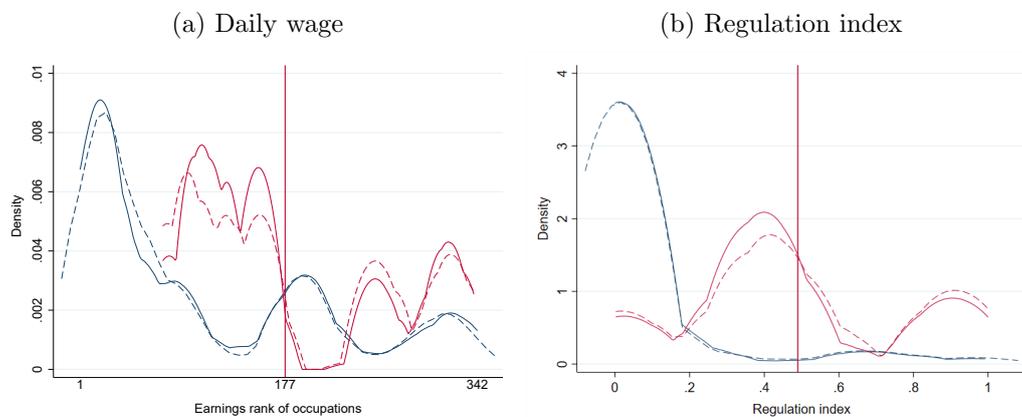
Figure 5 shows a very clear shift in the earnings and regulation distribution following transitions from non-regulated to regulated occupations. For example, non-EU immigrants move from cleaning jobs to working as nurses and doctors. This finding corroborates previous results showing positive employment and earnings effects after immigrants acquire recognition (Brücker et al., 2021). However, more important for our question, Figure 5 demonstrates that the earning and regulation distributions of non-EU immigrants pre- and post-reform are almost identical. This finding provides initial evidence against selection following the easier access to recognition, because non-EU immigrants move from the same low-paying occupations – which do not required recognition – to higher-paying regulated occupation pre- and post-reform.

6.1 Labor Market Outcomes Before Entering Regulated Occupations

We now use a regression framework to investigate more formally whether the reform created selection. We adopt the same DiD strategy using EU15 immigrants as the control group and estimate Equation 1 with employment status and employment characteristics (e.g., working hours, task, wage, degree of regulation) of the spell previous to the transition as the dependent variable. We include only those EU

³³We construct the earnings rank distribution by computing the average daily wage of Germans per occupation and ranking occupations by this value.

Figure 5: Occupational distributions of regulated occupations and pre-transition occupations



Notes: Figure 5 display the relative frequency of immigrants in different occupations by the earnings rank of an occupation based on natives' average daily wages (Panel a) and by the values of the regulation index (Panel b). We obtain the earnings rank of occupations by computing the deflated average daily wage of all Germans employed in each occupation during 2007-2017 and then rank occupations according to those wages. The ranking is plotted on the x axis. The density distributions represent the relative frequency of immigrants in the different occupations. Red lines are the density distribution in regulated occupations with the most applications; blue lines in occupations non-EU immigrants held before moving to regulated occupations. Solid lines are constructed from transitions to regulated occupations occurring post-reform; dashed lines, from those pre-reform.

Source: Integrated Employment Biographies

and non-EU immigrants who switched to a regulated occupation in our observation period.

Table 6, Column 1, shows that more non-EU immigrants switched from employment to a regulated occupation post-reform than before. This finding indicates that immigrants with strong labor market attachment benefit more from the reform. The characteristics of the last employment spell before transition (Columns 2-5) reveal only a small and marginally significant effect on the probability of having a part-time employment in the last spell before moving to a regulated occupation, while we find no effect on the probability of having a manual task, on the degree of regulation, or on the full-time wages (confirming the results in Figure 5). These findings indicate that while the reform led to an increase of non-EU immigrants entering regulated occupations, it did not change the quality of non-EU immigrants, as approximated by the characteristics of the job they held before they moved to regulated occupations.

6.2 Earnings in Regulated Occupations Before and After the Reform

The previous section showed that the reform did not lead to selection in terms of the labor market spells before moving to regulated occupations and Figure 5 suggested that non-EU immigrants switching to regulated occupations increased their wage. However, non-EU immigrants post-reform, relative to EU immigrants, may earn less in regulated occupations than pre-reform. A lower wage could occur if, for example, the reform reduced the signaling value of recognition, if employers believe that post-reform recognized certification is of lower quality, or if the quality of recognized certification is de facto lower.

To deal with this concern, we use the DiD strategy in Equation 1 to examine the wage difference between EU and non-EU immigrants within regulated occupations. If the wage difference between EU and non-EU immigrants does not change either pre- or post-reform, then the wage premium for non-EU immigrants working in a regulated occupation has very likely not changed.

Figure 6 shows the difference between EU15 and non-EU immigrants in log hourly wage from full-time employment in regulated occupations. Reported coefficients are

Table 6: Characteristics of the last employment spell before transition to regulated occupations with high number of applicants.

	(1)	(2)	(3)	(4)	(5)
	Employed	Part-time	Regulation index	Main task: manual	Full-time wage
Post*Non-EU	0.059 *** [0.012]	-0.020* [0.012]	-0.004 [0.008]	0.017 [0.010]	-0.006 [0.018]
Baseline (Non-EU)	0.65	0.45	0.06	0.70	6.59
R-squared	0.10	0.18	0.24	0.33	0.049
Individuals	24524	24524	24524	24524	16753
Observations	33039	33039	33039	33039	22470
Individual Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes

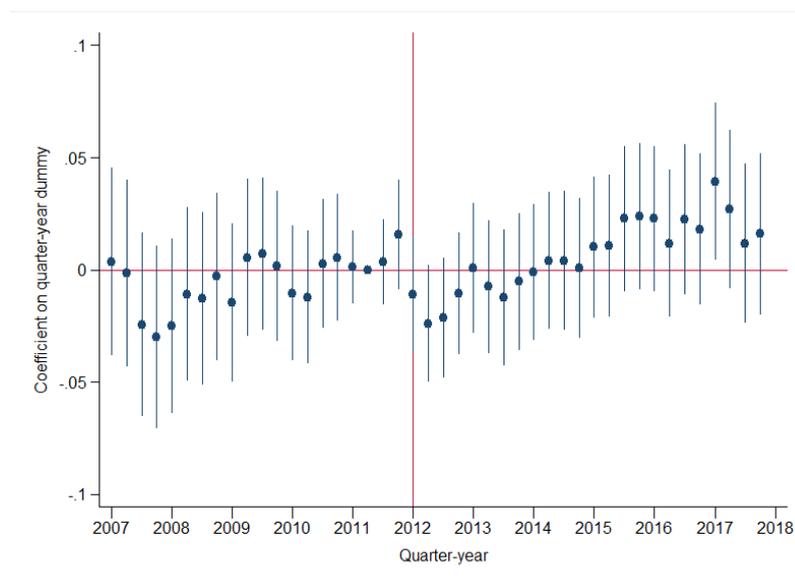
Notes: Table 6 reports the coefficient for regression models based on Equation 1 in which the outcomes are different characteristics of the last employment spell before moving to a regulated occupation with the many applications. Transitions *within* regulated occupations with many applications are excluded. column 1 the dependent variable is the probability of being employed in the spell before moving to a regulated occupation with the most applications. In columns 2-5, the dependent variables are constructed using the characteristics of the last employment spell (including if the individual is unemployed at t-1). Column 2 shows the probability that the last employment spell was part- or full-time. Column 3 presents the regulation index of the occupation before moving to a regulated occupation. In Column 4 the outcome is the probability that the main occupational task is manual (routine or non-routine) against non-manual. Column 5 shows the last full-time log hourly wage. The number of observations is lower in this case because only the last spells in full-time employment for which wage is available are included. Baseline is the average pre-reform for non-EU immigrants. Controls include sex, age, age squared, age at entry, age at entry squared, years in the register (and its squared transformation), nationality, educational level, year fixed effects, local labor market fixed effects, and dummies for all combinations of origin-destination occupations. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies

relative to the difference in 2010. The figure clearly shows no negative effect of the reform in the years after 2012. Indeed, the reform led to a slight increase in wages for non-EU immigrants.

Table 7 shows the regression estimation results for the analysis shown in Figure

Figure 6: Difference between EU and non EU-immigrants in full-time wages within target regulated occupations before and after the reform



Notes: Figure 6 displays the estimated coefficients and the 95% confidence intervals for a regression model of Equation 1, where the outcome is the log hourly wage of full-time employees in regulated occupations. Only full-time employees are included. The regression includes three-digit occupation fixed effects. Coefficients are estimated for each quarter before and after the reform. The baseline is March 2011 (one year before the reform). Each coefficient represents the difference in percentage points from the baseline difference in outcomes between EU15 and Non-EU.

Source: Integrated Employment Biographies (IEB).

6, both for the main control group (Column 1) and for the two alternative control groups used in Section 5.2 (Columns 2 and 3). Importantly, to control for immigrants' possibly sorting into different occupations in the post-reform period, we include three-digit occupation fixed effects. In the post-reform period non-EU immigrants employed full-time in regulated occupations experience a 2% increase in hourly wages relative to EU15 immigrants. The estimated coefficients using Germans and non-EU immigrants trained in Germany as alternative control groups are close in magnitude to the baseline coefficient and statistically significant. Column 4 shows the results using EU15 immigrants as the treated and Germans as the control, with the effect of the reform close to zero and not statistically significant.

Taken together, the results in this section show that for earnings in regulated occupations, the reform appears to have had no negative effects. Indeed, non-EU immigrant wages in regulated occupations slightly grew. Thus we argue that up to five years post-reform, neither non-EU immigrants with less ability selected into

Table 7: Effects of the Recognition Act on earnings in regulated occupations

	(1)	(2)	(3)	(4)
	EU15	Non-EU with domestic education	Germans	Germans
		Non-EU as treated		EU15 as treated
Post*Non-EU	0.019 * [0.011]	0.024 ** [0.009]	0.021 ** [0.008]	
Post*EU15				0.009 [0.008]
Baseline (Non-EU)	10.71	10.71	10.71	15.71
R-squared	0.57	0.46	0.42	0.42
Individuals	6902	10980	39828	38261
Observations	111643	160705	702385	678346
Individual controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes

Notes: Table 7 reports the estimated coefficients for different regression models based on Equation 1, where the outcome variable is log hourly wage from full-time employment. Only full-time employees are included. The first row indicates the control group used, the second row indicates the treated group. Individual controls include sex, age, age squared, years in the register, years in the register squared, age at entry, age at entry squared, nationality, and educational level. Year fixed effects, local labor market fixed effects, and occupation fixed effects are included. Each occupation dummy corresponds to one regulated occupation with the most applicants, defined by the 3-digit kldb1988 classification. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies

regulated occupations, nor that the employers valued the certificates less.

7 Effects on Employment in Non-Regulated Occupations

This section extends our main analyzes by investigating the reform effects on non-EU immigrants' employment in non-regulated occupations. Although entering these occupations do not require recognition, certification may signal productivity and thus facilitate access to employment. This signal may be an important device for employers to dissolve information asymmetries in the presence of foreign professional degrees.

For non-regulated occupations the identification of reform effects is not as straightforward as for regulated occupations, because recognition is not an access tool for these occupations. Therefore, an increase in immigrants' employment in non-regulated occupations is not necessarily linked to more recognized foreign certificates. Nevertheless, we apply the same identification strategy for both types of occupations, since an employment increase of non-EU immigrants in comparison to EU immigrants in non-regulated occupations after the reform would be the result of the reform. This result would be an important insight into the overall effect of a recognition reform although we cannot directly link it to more recognized certificates.

Column 1 and 2 of Table 8, which include results for all eligible non-regulated occupations, show that for non-EU immigrants the employment probability in a non-regulated occupation increases post-reform by 3 percentage points compared to EU immigrants – an increase of 6.8 percent. The next two columns differentiate between non-regulated occupations with a high/low number of applicants for recognition, where occupations with low application numbers serve as placebo occupations. If we observe an employment increase in these occupations, our estimates are likely to pick up a general trend in employment of non-EU immigrants, not a reform effect. For the non-regulated occupations with the most applications (Columns 3 and 4), the effect is also 3 percentage points, which corresponds to a much higher increase (14.8 percent) than for the entire sample. In contrast, the changes are almost zero in our placebo estimations for occupations with the fewest applications confirming that the employment increase results from increased recognition.

The finding that the reform also had an effect on non-regulated occupations indicates that non-EU immigrants indeed used the easier recognition procedure not only for mandatory certification but also for signaling and transparency and thus easier access to employment. Likewise, as the recognition reform even affected occupations for which recognition is not required (non-EU immigrants without recognition were allowed to work in these non-regulated occupations pre-reform), employers appear to value the recognition signal and higher transparency. One explanation may be that certification in a foreign language or the employer's not trusting its quality can use the recognition to dissolve information asymmetries. Alternatively, the successful recognition of home-country certification may motivate

Table 8: Effects of the Recognition Act on employment in non-regulated occupations eligible for recognition

	(1)	(2)	(3)	(4)	(5)	(6)
	All non-regulated		Non-regulated (many applicants)		Non-regulated (few applicants)	
Post*Non-EU	0.030*** [0.005]	0.031*** [0.005]	0.031*** [0.004]	0.033*** [0.004]	-0.001 [0.004]	-0.002 [0.004]
Baseline (Non-EU)	43.91	43.91	20.85	20.85	23.1	23.1
R-squared	0.008	0.057	0.004	0.049	0.002	0.057
Individuals	51237	51237	51237	51237	51237	51237
Observations	1174472	1174472	1174472	1174472	1174472	1174472
Individual Controls	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
LLM FE	No	Yes	No	Yes	No	Yes

Notes: The outcome variable is the probability of being employed in any non-regulated occupation (1st and 2nd columns), in non-regulated with high numbers of applications (3rd and 4th columns), or in non-regulated occupations with low numbers of applications (5th and 6th columns), as compared to being in any other state. Non-regulated occupations are vocational (*Ausbildungsberufe*). Each individual is assigned the value 1 if employed in an occupation in the group, and zero if in any other labor market condition. Employed individuals with invalid or missing values on the occupational code are excluded. The reported baseline is the average value of the dependent variable for the treated group (non-EU immigrants) at $t = 0$ (2007-2010). Only immigrants who were in Germany pre-reform are included. The sample is further restricted to individuals age 23-55 (to take into account the earlier acquisition of vocational training certificates) and only with an education level equal to vocational certification. Controls include sex, age, age squared, age at entry, age at entry squared, years in the register (and its squared transformation), nationality, educational level, year fixed effects, and local labor market fixed effects. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies.

non-EU immigrants to apply for a non-regulated occupation for which they are qualified. Unfortunately, disentangling these two channels is not possible with our data.

8 Discussion and Conclusion

Immigrants perform worse in the labor market than natives, likely because of the low transferability of home-country professional certificates. The formal recognition of professional certificates in the host country represents one policy for increasing their transferability. This paper investigates the effects of a large recognition reform in Germany on the labor market outcomes of non-EU immigrants. We find that the reform was highly effective. It increased both the recognition applications of

non-EU immigrants and their employment in regulated occupations, all of which require recognition. The reform also increased employment in the non-regulated occupations with the most recognition applications. These results are stable up to five years post-reform.

As the non-EU immigrants moved from low-wage and non-specialized occupations to higher-paid and regulated occupations, their increased employment in regulated occupations also improved their earnings. Furthermore, despite the larger inflow of non-EU immigrants into these occupations, the average wages for non-EU immigrants did not decrease post-reform.

Our results are highly valuable for policy makers worldwide, as many countries are considering facilitating access to recognition as a way to promote the integration of immigrants. Opponents of easing immigrants' access to recognition often argue that the quality in regulated occupations, for example the quality of health services, may decrease if more and possibly less qualified immigrants obtain access to recognition. However, our findings show that, as long as the recognition standards remain the same, an increase in the number of recognized certificates does not necessarily lead to lower quality. If the quality of recognized certification had declined post-reform, we would expect employers to have observed this decline and adjusted their labor demand for non-EU immigrants. Had this been the case, the reform effects on employment and wages would not have lasted for five years.

Our empirical strategy does not allow us to investigate in depth how the reform affected the employment and wages of German natives. However, as the percentage of natives who work in regulated occupations and their wages remained similar both pre- and post-reform, it gives a first indication that the reform did not harm their employment outcomes. The effects on employment are mainly in health sector occupations (a booming sector in Germany), thereby likely explaining why natives did not lose employment or earnings. Yet we also find effects of the reform in regions where the demand for health sector jobs is lower and for occupations outside the health care sector. Thus we argue that a recognition reform is effective not only in settings where demand for employment in regulated occupations is high. Moreover, even if the reform is effective only if the demand is high, the reform will raise overall welfare by increasing the supply of scarce human capital.

Taken together, our results point to the importance of removing formal barriers to the transferability of foreign-acquired human capital. Improving recognition procedures in terms of both the administrative burden and access to information may be a cost-efficient policy for integrating immigrants into their host country's labor market.

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Chapter 3

Can Tax Incentives Bring Brains Back? The Effects of Returnees' Tax Schemes on High-Skilled Migration in Italy

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Abstract

Brain drain is an increasingly relevant concern for many countries experiencing large emigration rates of young and skilled individuals. In response, governments have designed fiscal incentives to attract high-skilled expatriates and foreigners. Yet, empirical evidence on the effectiveness of tax incentives in attracting high-skilled migrants is limited. In this paper we focus on the Italian 2010 tax scheme, which granted a generous income tax reduction to high-skilled expatriates in a context of increasing brain drain. Eligibility for the scheme required a college degree as well as being born after January 1st, 1969, which creates suitable quasi-experimental conditions to identify the effect of tax incentives. Using a Diff-in-Diff strategy and administrative data on return migration, we show that eligible individuals are 50-60% more likely to return post-reform relative to the pre-reform period. Additionally, using social security data from Germany - the main origin country of returnees, we find homogeneous effects across the wage distribution, suggesting that mobility responses to tax incentives may be a broader phenomenon not limited to top earners.

Key Words: Emigration, Tax incentives, Brain Drain, Return migration

JEL Codes: H24, J61

1 Introduction

Emigration of young and high-skilled individuals is an increasingly relevant concern for many countries (Docquier and Rapoport, 2012; Docquier et al., 2014). “Brain drain” is especially detrimental if outflows are not compensated by an equivalent inflow of high-skilled workers from abroad, determining a net loss in countries’ human capital (Boeri et al., 2012). Once a phenomenon mostly characterizing developing countries, in the last decade there has been an increase in brain drain from developed countries such as Southern European countries. These experienced substantial emigration flows especially after the Schengen treaty introduced free mobility of labor within the European Union.

The spike in emigration flows has motivated many European governments to implement measures to counteract brain drain. Specifically, governments started to provide fiscal incentives to attract expatriates and foreign nationals, by granting substantial tax discounts to high-skilled individuals who move their residence to the country². Yet, there is limited empirical evidence on the efficacy of tax incentives in attracting high-skilled migrants, and particularly on their effectiveness to mitigate brain drain.

What are the effects of granting substantial tax discounts to high-skilled immigrants and return migrants, especially in a context of high emigration? On the one hand, a growing empirical literature finds large mobility responses of individuals in response to fiscal incentives (see Kleven et al. (2020) for a survey), suggesting that preferential tax schemes can potentially be an effective tool to (re-)attract high-skilled individuals. Furthermore, recent empirical papers on brain drain within the EU estimate negative spillover effects of emigration on sending countries’ firm creation (Anelli et al., 2020) and TFP (Giesing and Laurensyeva, 2017). These negative effects may be counterbalanced by increased return migration, which could bring positive spillovers in the labor market and potentially contribute to economic growth.

On the other hand, if tax incentives are mainly taken up by infra-marginal individuals who would have returned anyway, or if they only attract low-skilled emigrants, or if a large fraction of these benefits are passed through to employers

²Preferential tax schemes have been introduced in the Netherlands (1985), Denmark (1991), Finland (1999), Sweden (2001), France (2004), Spain (2005), Portugal (2009), and more recently in Italy (2011). Source: Kleven et al. (2020).

by reducing returnees' gross wages, then the cost of providing tax incentives may exceed the benefits from a public finance perspective and create dead-weight losses. Moreover, incentivizing returns may have the unintended consequence of inducing high-skilled individuals who would have otherwise stayed to *emigrate*, a fraction of which may end up staying in destination countries, thus exacerbating brain drain. Lastly, as high-skilled migrants are likely to come from relatively advantaged family backgrounds, these policies might increase income inequality in the sending countries both by reducing taxes for richer returnees, while leaving taxation unchanged for all other groups.

In this paper, we aim to shed light on the effectiveness of tax incentives to mitigate brain drain. Our empirical analysis focuses on Italy, which has experienced a steady increase in the outflows of young and high-skilled individuals during the last decade. As Figure 1 shows, emigration flows have increased dramatically since the onset of the Great Recession and especially after the Sovereign Debt crisis hit in 2011, while return migration inflows, which in the early 2000s were nearly balancing outflows, were systematically lower. Furthermore, such a spike in outflows was concentrated among young (under 40 years old) and highly educated (college graduates) individuals, as shown in Figure 2.

Besides being characterized by high emigration flows, Italy provides a suitable empirical setting to study the effects of tax incentives on migrations. Indeed, two main challenges exist when trying to estimate the effects of tax incentives on individual mobility (Kleven et al., 2020). The first is the lack of administrative data on mobility, especially on emigration: while most countries accurately record immigration flows, data on emigration (and return migration) is usually more scarce and less reliable. Italy is an exception in that it collects detailed administrative records on emigrants and returns, which we discuss in Section 3. The second is the scarcity of plausibly exogenous tax variation to identify causal effects: as location choices of individuals are likely to be correlated to unobserved economic conditions of destination and origin locations, and tax schemes are often implemented in response to economic conditions, selection and omitted variable bias might hinder identification of the effects of tax incentives.

In late 2010, the Italian government introduced a preferential tax scheme for young

high-skilled expatriates who moved their residence back to Italy. Specifically, this scheme granted a generous 70-80% reduction³ of taxable labor income for expatriates who returned to Italy starting from 2011, but only if they had a college degree and they were born after January 1, 1969 (i.e. if they were under 41 years old in 2010).⁴ The joint presence of these eligibility requirements create suitable quasi-experimental variation to identify the elasticity of return migration to tax incentives.

In our empirical analysis we exploit these quasi-experimental conditions in a difference-in-differences (DiD) strategy. Using administrative data on international migration of Italian citizens, we find that eligibility for the 2010 tax scheme significantly increased return migration rates of young and high-skilled Italian expatriates by up to 60%, suggesting that about 40% of returnees eligible for the tax incentives would not have returned absent the incentives. The estimated effect is driven by Italians returning from other European countries and within the Schengen area. Further, using social security data on the universe of Italians workers in Germany (which is the main origin country of Italian returnees), we estimate a similar effect on the probability that Italians expatriates in Germany leave the registry, and we uncover relatively homogeneous effects across the wage distribution of Italian workers in Germany, suggesting that mobility responses to tax incentives may be a broader phenomenon not limited to top earners.

Our paper is among the first to study the effect of tax incentives on return migration, particularly in a context of mass high-skilled emigration. The closest papers to ours are [Del Carpio et al. \(2016\)](#), who study the effects of a program to attract Malaysian nationals living abroad that offers tax deductions upon return, and [Kleven et al. \(2014\)](#), who focus on a 1991 preferential tax scheme in Denmark for high-earner immigrants and estimate a large elasticity of migration to tax incentives for foreigners. More generally, our paper is related to three main strands of literature. First, we contribute to the literature on the mobility responses to taxation. Previous research has focused on the mobility of top earners either within national boundaries ([Agrawal and Foremny, 2019](#); [Moretti and Wilson, 2017](#); [Schmidheiny and Slotwinski,](#)

³The reduction was 70% for men and 80% for women.

⁴Eligibility also required at least 2 years of stay abroad and 2 years of *pre-residence* in Italy before expatriating. While all EU citizens were eligible for the scheme, this latter requirement limits dramatically the fraction of eligible foreign nationals. For this reason, in this paper we focus only on Italian citizens.

2018; Liebig et al., 2007; Young and Varner, 2011) or across countries (Akcigit et al., 2016; Kleven et al., 2014, 2013; Muñoz, 2019) in response to tax changes. We show that a tax scheme which does not exclusively apply to top earners can also trigger migration responses. Second, it is related to the literature on the determinants of return migration (Dustmann and Görlach, 2016; Adda et al., 2016). We complement this literature by investigating how migrants abroad respond to a large shock in net wage differentials between their home country and the destination countries. Last, we contribute to the empirical literature studying the economic effects of brain drain (Anelli et al., 2020; Giesing and Laurensyeva, 2017), brain return (Mayr and Peri, 2009) and brain gain (Fackler et al., 2018), and to papers investigating the role of migration policies on the mobility of high-skilled individuals (Kato and Sparber, 2013; Kerr et al., 2017; Czaika and Parsons, 2017; Boeri et al., 2012). We provide the first evaluation of a large policy predominantly targeting high-skilled nationals residing abroad.

The remainder of this paper unfolds as follows. In Section 2 we document the recent trends in Italian emigration and we illustrate the main features of the tax scheme for returnees. In Section 3 we describe our data and we show some descriptive statistics. In Section 4 we explain our identification strategy and in Section 5 we present our results. Section 6 concludes with a discussion and future extensions to this work.

2 The Italian setting

2.1 Emigration of young and high-skilled individuals

Already for the late 1990s, Becker et al. (2004) documented the peculiarity of Italy as a “net exporter of brains” among EU countries: “the tendency of Italian college graduates to move abroad does not seem to be balanced by a corresponding tendency of foreign college graduates to move into the country”. Using data from the OECD DIOC on the stock of immigrants and natives by education level in 2010, in Figure 3 we confirm that this picture is still accurate one decade later: while the share of high-educated Italians living abroad is comparable to many other European countries such as the UK, Belgium, France and Germany, Italy stands out by ranking

at the bottom of OECD countries in terms of the share of high-educated among its foreign-born population. Furthermore, survey evidence from [Saint-Blancat \(2019\)](#) reveals that such a brain imbalance is especially pronounced among scientists and researchers, thus confirming the severity of the Italian brain drain at the very top of the skill distribution.

If the stocks in 2010 already reveal the severity of the Italian brain drain, the picture becomes much worse if we consider the migration flows in the following decade: as shown in [Figure 2](#), the share of new emigrants among young and tertiary educated Italians has more than tripled since 2010, while the increase was much smaller among older and lower educated individuals. The preferred destination is Western Europe – in line with the easiness to travel and relocate that EU citizens enjoy within the Schengen area – and Germany is among the top-3 destination countries of recent emigrants ([Figure 5](#)) and the main origin country of return migrants ([Figure 6](#)).

2.2 Tax incentives for returnees

In this context of brain drain, Italy has tried to reverse the negative trend by granting fiscal incentives to high-skilled return migrants. In late 2010, the Parliament approved Law 238/2010 “Controesodo” (hereby the “2010 Reform”) which introduced, starting from 2011, the first tax incentives applying to all return migrants regardless of their occupation, as long as they held a university degree⁵ and they were born on or after January 1st 1969.⁶ In addition, eligibility required a EU citizenship, a minimum of 2 years of *pre-residence* in Italy before emigration, as well as at least 2 years of stay abroad before returning. Under this scheme, a fraction between 70% (for males) and 80% (for females) of labor income⁷ of returnees is exempted from individual income taxation, starting from the year of return to Italy and for at least 2-4 years.⁸ Subsequent laws (in 2015 and 2019), which applied to individuals who

⁵Emigrants who obtained a university degree abroad are also eligible for the tax scheme as long as they fulfill the length of stay requirement

⁶Back in 2003, the very first tax scheme has been implemented for researchers and university professors relocating to Italy. This scheme grants a 90% income tax exemption for 3 years (later extended to 4 and then to 6 years), and it is still in place as of today. The main eligibility criterion, besides holding a research position, is the length of stay abroad, two years at least.

⁷Specifically, the exemption applied to employee and self-employed labor income as well as to business income.

⁸More precisely, the law initially stated that the incentives were to be in place until December 31, 2013, which is about 5 years from when the Law was first discussed in the Parliament (January

returned starting from 2016, modified the percentage of exempted income and also removed the birth-cohort requirement (which by 2016 however it was not as binding as before), although they still targeted high-skilled individuals.

Figures 7 and 8 summarize all the relevant characteristics of the tax schemes, by showing the timing, size and main eligibility criteria of the tax schemes (Figure 7) and the duration of the incentives (Figure 8). Figure 7 shows that, while until 2010 young high-skilled returnees were subject to full taxation of their income, for those who returned starting from 2011, only a fraction between 20-30% of their income is taxable, resulting in dramatically lower average and marginal tax rates (which we simulate and show in Figures 9a and 9b). This advantageous scheme applied to college graduates born on or after 1969 as long as they returned to Italy until 2015. From 2016 onward, a modified scheme was applied, which was less generous in terms of the percentage of tax exemption (initially 70% but then reverted to 50% in 2017) but also more straightforward and generous in terms of the duration of the incentives (5 fiscal years regardless of the year of migration). In fact, while the duration of the 2010 scheme was eventually extended until 2017 (solid triangles in Figure 8), at the time when most individuals made their migration decisions, the duration was expected to be lower than 5 years (as shown by the light triangles).

The focus of this paper is on the 2010 Reform (the Controesodo Law) for several reasons. First, it was the first and most substantial set of tax incentives for high-skilled returnees, which generated a dramatic drop in the fraction of the returnee's income subject to taxation, and applied to all employed and self-employed workers regardless of their occupation. Second, the joint presence of a birth-cohort threshold as well as the educational attainment requirement provides a suitable quasi-experimental setting.⁹ Finally, as we observe the education level and the birth cohort of return migrants, we are able to identify their eligibility in the data. For these reasons, in our Difference-in-Difference strategy, the pre-period will be until year 2010 while the

20, 2009). As the legislative process took almost two years, in late 2011 the incentives were extended until December 31, 2015. Then, in late 2014 they were further extended to December 31, 2017, but this applied only to those who returned to Italy no later than December 31, 2015, as those who returned afterwards were subject to the new regime of D.Lgs. 147/2015.

⁹While the 2015 Reform eliminated the birth-cohort requirement, we still include those individuals born before 1969 who returned after 2015 in the control group, in order to have consistent eligibility requirement throughout the pre- and post-period. Results are nonetheless very similar if we include them among the treated, which suggests that the birth-cohort requirement was not as binding anymore in 2015 as it was in 2010.

post period will be from 2011 onward.¹⁰

The 2010 Reform provides a generous tax discount which is not limited to high earners. This is important since these incentives specifically targeted younger individuals, who are often in the early stages of their careers and thus not necessarily high earners. Figures 9a and 9b shows the actual reduction in the average and marginal income tax rates for different levels of gross earnings (from labor incomes only), taking into account the no-tax area as well as all the standard deductions of the Italian tax system. The graphs show that the incentives are sizeable for all income levels: the scheme reduces the average tax rate by 28-33 percentage points and the marginal tax rate by 34-37 p.p. for all taxpayers above the extended non-tax area created by the tax incentives, which is slightly above 50,000 euros.¹¹ The largest reduction in the marginal tax rate occurs for an individual earning between 31,000 and 42,000 euros (which is close to the starting gross wage of high-skilled individuals in Italy), as their marginal tax rate would drop from 39% to zero. Appendix Figures B.1 and B.2 show a similar picture if we include the compulsory social security contributions paid by the employee.

3 Data

3.1 Administrative data on migration flows (Istat)

The Italian National Statistical Institute (Istat) collects information from civil registries on all residence changes, both within Italy and to/from abroad, which are among the best available measures of migration flows. These administrative, individual-level records include information on year of migration, origin and destination (Italian municipality or foreign country) as well as several demographic variables such as date of birth, birthplace, gender, education level, citizenship and marital status

¹⁰We use all the data until the last available year, which is either 2017 or 2018. Results are robust to excluding years 2016 and onward, when the 2015 Reform with different eligibility requirement and exempt income percentage was in place, as well as to excluding year 2011, as some minor eligibility requirements were not clear until mid-2011, when *Agenzia dell'Entrate* (the Italian fiscal authority) issued some clarifications.

¹¹For simplicity, in the figures we are considering an average between the tax exemption for males (70%) and for females (80%), i.e. 75%.

at the time of migration.¹² In this paper we use an aggregate version of these data. Specifically, we obtained data from Istat on the number of Italian citizens returning to Italy from abroad, by year of migration (2002-2018), birth cohort (5-year groups), education (less than high school, high school and college), sex, foreign country of origin and a foreign-born indicator.

The Istat data are based on the enrollment and dis-enrollment from the *Anagrafe degli Italiani Residenti all'Estero* (AIRE; Registry of Italians Residing Abroad).¹³ Italian citizens are required by law to change their residence whenever they migrate abroad for more than 6 months, which involves a dis-enrollment from the civil registry of their municipality of origin and the enrollment in the AIRE registry. The main benefit of enrolling is that foreign income is not subject to income taxation in Italy, in addition to access to voting from abroad and consular services. Once they return to Italy, they are dis-enrolled from the AIRE registry and enrolled in the civil registry of their destination municipality, which is our measure of return migration. Istat collects all these individual records and aggregates them into emigration (from Italy to abroad) and return migration (from abroad to Italy) flows.

Two main limitations characterize the Istat data. The first is under-reporting. In fact, despite the substantial benefits to enroll in the AIRE registry, there is evidence that a large fraction of Italians do not enroll when they move abroad (Anelli et al., 2020), and, consequently, they do not appear in the return migration data. In Section 3.3 below we assess the extent of under-reporting by comparing the return migration flows recorded in the Istat data with the corresponding statistic from Destatis, the German statistical institute. While this is an important limitation of the Istat data, it does not constitute a problem for our identification strategy as long as it is not differential between eligible and non-eligible individuals, as we discuss in Section 4.

A second limitation of the Istat migration data is that we do not have direct information on the eligibility for tax incentives. Nonetheless, we can infer their eligibility quite precisely from their birth cohort, sex and education level.¹⁴ Eligibility

¹²Access to the full individual-level microdata is restricted. Researchers can apply for access, which must happen in the Istat cold rooms in Italy with severe restrictions.

¹³We refer to Anelli and Peri (2017) and Anelli et al. (2020) for a discussion about these data.

¹⁴While education is self-reported at the time of migration, there is no incentive to misreport the educational attainment to qualify for the tax scheme, as the residence change form which the Istat data is based on has purely statistical and registry purposes, and it is not used by the tax administration to determine actual eligibility, which instead relies on information from employers.

for the 2010 Law requires, in addition to being born after 1969 as well as having a college degree, to have resided in Italy for at least 2 years and then to have spent at least 2 years abroad. While we do observe birth cohorts and educational level precisely enough to identify eligible and non-eligible individuals, unfortunately we do not observe whether they had resided in Italy before emigrating, nor how much time they spent abroad, nor whether individuals are in the labor force and thus earn a positive income or not. While these should not create threats to identification (as long as they are not differential between the treated and control groups), it is likely that measurement error could attenuate our estimates. For these reasons, we limit our analysis to Italian citizens born in Italy, as by definition they must have spent some time in Italy before going abroad, and to individuals 25-64 years old, as they are more likely to be in the labor force. Furthermore, we drop individuals with educational level below high school (around 10%) to have a better control group for college graduates, which are the affected group by the reform.

Figure 6 shows the number of returnees over time for each of the top-5 countries of origin. Germany is the top origin country during the entire period, accounting for about 4,000 individuals returning each year, followed closely by the United Kingdom and Switzerland especially in later years. The predominance of Germany in the migration flows of Italians motivates our focus on the Germany-to-Italy migration flows.

3.2 German social security data (IEB)

To complement our analysis, we use German social security data (Integrated Employment Biographies; henceforth IEB) from IAB to investigate the effects of eligibility for tax incentives for Italian citizens who are abroad. For this purpose, Germany is a particularly suitable destination country for at least three reasons. First, it is the second largest Italian community abroad (second only to Argentina) with over 800,000 registered Italians (AIRE 2018). Second, together with the UK it is the most frequent destination for recent Italian emigrants (Figure 5) and the top country of origin of returnees (Figure 6). Last, migration pattern of Italian to and from Germany strongly resemble the overall outflows.

For this paper, we obtained access to the universe of Italian citizens¹⁵ in the German social security data from 2000 to 2018. The data include detailed information on their employment and unemployment spells, including wage, occupation, sector, firm characteristics and precise location of work. The main limitation of these data is that we do not observe individuals migration behavior, that is where they move once they leave Germany. For this reason, we assume that Italian citizens between 25 and 55 years old who leave the register (excluding working students and retired individuals) are return migrants to Italy. In Section 3.3 below we corroborate the soundness of this assumption by comparing the annual flows of Italians leaving the social security registry with official migration statistics from Germany and Italy.

In Table 1 we show the main characteristics of two subgroups of interest, namely Italians 25-64 years old who entered to and who exited from the social security registry between 2000-2017. The first group is relevant because it includes migrants who arrive to Germany before, during and after the recession. The second group is the group of interest, mostly target of the reform. Italian migrants are more likely to be male, around 20% has a college degree or higher certificate. The average duration of leavers is around 2.5 years, which suggests that migration is rather temporary and possibly circular.

3.3 Comparison between Istat and IEB data

In Appendix Figure B.3 we compare migration flows from Germany to Italy as proxied by three different data sources: the Italian migration data (Istat), the IEB data on the Italians that leave the German social security registry, and the German migration data (Destatis). Two main takeaways emerge from this exercise. First, a comparison between the migration flows recorded by Destatis (red-circles) and Istat (green-triangles) suggests that the under-reporting in the Istat data is substantial, as found by [Anelli et al. \(2020\)](#). Nevertheless, since the Destatis flows include all non-German nationals while the Istat data includes only Italian citizens, the difference between the two series is an upper bound of the true difference. Second,

¹⁵We consider only individuals with Italian nationality as the most frequent nationality value. We therefore exclude individuals in the German social security records that have only one or few spells with Italian nationality, as these individuals may be naturalized as Germans and therefore unlikely to have emigrated from Italy in the previous years.

a comparison between the Destatis data and the IEB data (dotted blue-diamonds) reveals that, despite the Italians who leave the IEB data are much more than the Italians who return to Germany as per the Istat data, the leavers flow is still smaller than the migration flows recorded by Destatis. This suggests that, even if some of the Italians who disappear from the German social security data might have migrated elsewhere than Italy, or perhaps might have exited the labor market without leaving Germany, this is unlikely to be a major issue in our setting. If anything, the IEB might instead be missing some Italians (e.g. students) who might have been in Germany and then returned to Italy without ever appearing in the social security data.

Overall, while both data sources have limitations and thus both imperfect proxies for return migration, these limitations are nonetheless very different in terms of their underlying causes (under-reporting for the Istat data and imperfect proxy of return migration for the IEB data). Therefore, it is reassuring that we find very similar result in our empirical analysis, as we show in Section 5.

3.4 Additional migration data

While Istat data provide a measure of migration flows, there is no information on the stock of Italians abroad. Therefore, we complement the Istat data with migration statistics from destination countries using the *OECD Database on Immigrants in OECD and non-OECD Countries* (DIOC) as well as the IAB Brain Drain dataset (Brücker et al., 2013), which allow us to measure the stock of Italian emigrants in each destination country by educational group. The OECD and IAB Brain Drain datasets collect information on migrant stocks from national censuses and are disaggregated by gender, education and age.

4 Empirical strategy

4.1 Effects of tax incentives on return migration

To study the effect of tax incentives for high-skilled immigration, we rely on the quasi-experimental conditions created by the 2010 Reform which introduced a

substantial income tax incentive to high-skilled returnees. Specifically, this reform granted an exemption of taxable labor income of 70% to male and 80% to female college graduates who returned starting from 2011 but only if they were born after January 1, 1969.

To identify the effect of tax incentives, we use a Difference-in-Difference (DiD) strategy in the spirit of [Kleven et al. \(2014\)](#). Let $N_{c,e,t}$ be the number of returnees in birth cohort c , with education level e relocating to Italy in year t . Let $Treated_{c,e} = 1(c \geq 1969) * 1(e = college)$, i.e. indicating the groups eligible for the tax incentives, and $Post_t = 1(t \geq 2011)$, i.e. indicating the years when tax incentives were in place. Then, we estimate a simple DiD model:

$$\log N_{c,e,t} = \lambda_t + \gamma Treated_{c,e} + \eta Treated_{c,e} * Post_t + \epsilon_{c,e,t} \quad (3)$$

where λ_t denotes year fixed effects. Under the parallel trend assumption, namely, that absent tax incentives the eligible and non-eligible groups would have had similar trends in the likelihood of returning, η identifies the reduced-form, intention-to-treat (ITT) effect of eligibility for tax incentives on return migration.

In Section 5 we present the results of estimating Equation (6) and we show some visual checks on the parallel trend assumption. However, several threats to validity exist, which would pose a challenge to a causal interpretation of the estimated reduced-form effect. For instance, if labor demand for college graduates in Italy was less impacted than demand for high school graduates by the Sovereign Debt crisis in 2011, we would overestimate the effect of tax incentives. Further, group-specific labor demand shocks may have also differentially affected emigration flows, mechanically increasing returns among these broadly defined groups. For these reasons, in a robustness check we exploit the fact that eligibility combines an age as well as an education requirement to estimate a Triple DiD specification:

$$\log N_{c,e,t} = \lambda_t + \gamma Treated_{c,e} + \eta Treated_{c,e} * Post_t + \mu_c + \mu_e + \mu_{c,e} + \mu_{c,t} + \mu_{e,t} + \epsilon_{c,e,t} \quad (4)$$

in which we allow young individuals and college graduates to be on different trends by including birth cohort-by-year ($\mu_{c,t}$) and education-by-year ($\mu_{e,t}$) fixed effects.

Under this Triple DiD specification, the parallel trend assumption requires that the relative outcomes between college and high school graduates among the eligible cohorts (those born in or after 1969) would have been on the same trend as the relative outcomes among the non-eligible cohorts (born before 1969).

Finally, as the number of returnees mechanically increases with the stock of individuals abroad, we also estimate a DiD regression on the return rates, i.e. the number of returnees in each year t as a percentage of the stock of Italians in destination countries in 2010.

4.2 Effects on leavers from German social security data

As a second approach to evaluate the effects of tax incentives on return migration, we use the universe of Italian migrants working in Germany. This second approach is complementary to the first along two dimensions. First, we are able to precisely identify the eligible group, both for stayers and for migrants who leave the German labor market in each year. We can therefore replicate the analysis with Italian migration data using individual-level data on Italian citizens who leave the German social security register. Second, exploiting the panel structure of the data and the detailed labor market information we can characterize the last spells before leaving and investigate whether tax incentives changed the selection of return migrants and the length of stay in Germany.

To investigate the effect on returns, we estimate the following equation:

$$Pr[L_{it}] = \alpha + \gamma Treated_{c(i),e(i)} + \beta(Treated_{c(i),e(i)} * Post_t) + \psi' X_{it} + \lambda_t + \epsilon_{it} \quad (5)$$

where $Pr[L_{it}]$ is a dummy for individual i leaving the German labor market in quarter t , $Treated_{c(i),e(i)}$ is a dummy for being eligible to the tax incentives (born after 1969 and with tertiary education degree), $Post_t$ is a dummy for the timing of the Controesodo Law (equal to 1 for 2011 and after), X_i is a vector of individual-level controls, λ_t are year fixed effects and ϵ_{it} is the error term. The parameter of interest is β which identifies the post-reform difference in the probability of leaving between treated and control migrants relative to the pre-reform difference. Similarly to the first approach, we also estimate a Triple DiD combining the two eligibility criteria

(education and birth cohort). Individual controls are gender, age at entry in the register and birth cohort. One threat to identification in the German data is that changes in return migration might be related to shocks specific to Germany that affect all migrants. For this reason, we also use an alternative control group, Spanish nationals born on after 1969 and with college education to estimate Equation 8. Finally, to investigate changes in the characteristics of leavers, we consider only the last employment spell of migrants who leave the register and estimate Equation 8 using the log length of stay in Germany as outcome. In all regressions, standard errors are clustered at the individual level to take into account the panel structure of the data.

5 Results

5.1 Visual evidence on return migration by eligibility status

Figure 10 shows the evolution of return migration flows by eligibility for tax incentives. Two main trends emerge from the graph. First, the treated and control series were on parallel trends before 2011, providing confidence on the validity of our parallel trend assumption. Second, the slope of the return migration flows of eligible individuals markedly increase after 2011, when the eligible and non-eligible series start diverging. In Figure 11, we further breakdown the non-eligible group by plotting separate series for college graduates born before 1969 (blue-diamonds), as well as high school graduates born on or after 1969 (yellow-circles) and before 1969 (green-circles). It is reassuring that all groups are on fairly parallel trends before 2011, and none of the control groups displays the slope change experienced by the eligible group after 2011.

To give a sense of which cohorts are driving the divergence shown in the previous graphs, in Figure 12 we plot the age distribution of returnees, separately for four groups: college graduates before (average 2006-2010) and after (average 2012-2016) the reform, as well as high school graduates before and after the reform. On the x-axis we have each 5-year birth cohort intervals (from older to younger cohorts), while the y-axis shows the yearly average number of returnees from each cohort. The figure reveals several patterns. First, return migration tends to peak at about 35

years old (consistently with the German data shown in Table 1), suggesting that the age requirement of the reform (being born after 1969) was binding for a non-negligible share of returnees. Second, while there is a mechanical leftward shift for both college (eligible) and high school (non-eligible) graduates after the reform due to the fact that we observe return migration 6 years after relative to the pre-reform period, the former (college graduates) shows a considerable excess mass for the cohorts born after 1969, i.e. to the right of the vertical line.

5.2 DiD effects of eligibility status on return migration

We then confirm the visual evidence presented above by estimating a Difference-in-Difference model. In Table 2 we show the results of a simple DiD regression with multiple periods by collapsing data into eligibility status by year cells, thus mimicking the trends shown in Figures 10 and 11. In Column 1 we pool all control groups into the non-eligible group, while in Columns 2 and 3 we use only college graduates born before 1969 and high school graduates born on or after 1969 as control groups respectively. The estimate confirms the graphical evidence shown in Figure 10: the coefficient of the interaction term $Treated_{ce} * Post_t$ is positive and statistically significant, suggesting (after the exponential of η) that eligible individuals are between 56-67% more likely to return in the post period, depending on the control group considered. This translates to a fraction of marginal individuals of about 35-43%, i.e. the percentage of eligible individuals who returned after 2011 that would not have returned absent the tax incentives.

In terms of elasticity of return migration with respect to the average net-of-tax rate, the estimated η translates to a flow elasticity of between 1.2-1.4.¹⁶ Compared to the literature on migration responses to taxation, our estimates are comparable to the short-term flow elasticities for foreigners estimated by Kleven et al. (2014), but they are larger than their estimates for expatriates returning to Denmark. This is consistent with Italian tax incentives mostly targeting Italian expatriates, as Italy is experiencing a brain drain which was not the case in the Danish context.

In Table 3 we estimate the same specification, but the outcome variable is now

¹⁶We consider a reduction in the average income tax rate of 30 p.p. corresponding to a gross income of 75,000 euros (see Figure 9a). As the average net-of-tax rate before the incentives is about 0.32, we get $\log(1.67)/\log(0.98/0.68) \approx 1.4$.

return migration from Germany only. The results are quantitatively similar to the estimates pooling all countries of origin shown in Table 2, thus confirming that the trends in return migration from Germany by eligibility status resemble the overall trends.

One may worry that treated individuals could have been on different trends than non-eligible individuals after the reform, e.g. due to a differential impact of the Great Recession on demand for college graduates relative to high school graduates. As eligibility in our setting is based on both age and education, a Triple DiD allows us to include different intercepts by for each birth cohort and education level as well as differential trends by education and by cohort by including their interaction with year fixed effects (as shown in Equation 7). The results are presented in Table 6, Column 4, which shows a similar estimate of the key interaction term, suggesting again that treated individuals are about 57% more likely to return after the reform.¹⁷

In Tables 4 and 5 we estimate a DiD regression where the outcome variable is the return migration rate, i.e., the flow of returnees relative to the stock of Italians abroad as of 2010, rather than the log count of returnees, again both pooling all countries of origin (Table 4) and using only Germany (Table 5). This specification has several advantages. First, it allows us to compare the results estimated with the German social security data, where the outcome variable is the probability of leaving the registry (which proxies the probability of return), which we discuss in Section 5.3. Second, as the return migration flows are a function of the stock of Italian expatriates in foreign countries, it is important to account for the distribution of eligible vs non-eligible individuals abroad before the introduction of tax incentives, which we measure using the OECD DIOC data on the stock of Italian immigrants from destination countries Censuses as of 2010. Reassuringly, we estimated effects are similar to the estimates on the log counts: eligibility increases the return migration rates from all countries of origin by 43-51% relative to the baseline, and return migration rates from Germany by 29-38%, depending on the control group used.

Last, to further understand which groups of individuals and countries of origin are driving the estimated effects, in Figure 13 we show some heterogeneity by estimating

¹⁷In the same Table, Columns 2 and 3, we also show that the results are robust to excluding years 2016-2018, when returnees are subject to the new regime of the 2015 Reform, as well as to the exclusion of year 2011, as some eligibility requirements of the 2010 Reform were not clear until mid-2011.

separate regressions for each demographic group that we observe in the Italian data. While for women we estimate a slightly larger effect (consistent with the larger incentive), the coefficients for men and women are not statistically significantly different from each other. Finally, we plot the coefficients for the top countries of origin of Italian returnees. We estimate the largest and most precise effects on return migrants from other EU countries, such as Germany, Switzerland, UK, France and Belgium, consistent with the fact that expatriates to countries not distant from Italy are more responsive to the shock in net wage differentials created by the tax incentives.

5.3 Effects on the probability of leaving the German registry

In this section we present results of estimating Equation 8 with the IEB data on Italian workers in Germany. The outcome variable is the individual probability of leaving the register in year $t + 1$, conditional on being in the register in year t . Treated and control groups are constructed in the same as for analysis with the Italian migration data.

In Figures 14 and 15, we first show event-study graphs by plotting the coefficients of the interaction between the Treated dummy and year FEs. In the top graph we include all Italian workers while in the bottom graph we only include workers whose last spell in the data is an employment spell. While we do not see any pre-trend prior to 2011, the probability that the eligible group leaves the registry is significantly higher after 2011 relative to the controls, regardless of whether we use all control groups, high school graduates born on or after 1969 or college graduates born before 1969. The estimated effects are still significant but about half in size when we focus on employed workers only, which is not surprising, as it is likely that Italian workers who leave the country might transition through an unemployment spell first.

Table 7 shows the corresponding DiD results, pooling all control groups in Column 1, and separately for the two usual control groups, namely college graduates born before 1969 (Column 2) and high school graduates born on or after 1969 (Column 3) as control groups respectively. Similar to the event study graphs, the table is organized in two panels: in the top Panel (A), we include all workers while in the bottom Panel (B) we only include workers whose last spell in the data is an employment spell. We find a positive and statistically significant effect of the tax

scheme on the probability of leaving the register. For Panel A, pooling all control groups in Column 1, we estimate a 1.1 percentage point increase in the probability of leaving the register after the 2010 Reform for the eligible group relative to the controls, which corresponds to a 32% increase relative to the baseline. The coefficients are similar by using the young high school graduates (Column 2) and the college graduates born before 1969 (Column 3) as control groups, with the effect ranging between 29-35% of the baseline probability of leaving. Reassuringly, these effects are remarkably similar to the ones we showed in Tables 3 and 5 using the Italian data on return migration from Germany, which suggests that the probability of leaving the register is likely a good proxy for the probability of returning to Italy.

Finally, to shed some light on the characteristics of workers at the margin of migrating in response to the tax incentives, we estimate the DiD model separately for different subgroups of employed workers. Figure 16 show graphically the results of this exercise. Interestingly, we find relatively homogeneous effects across the wage distribution of Italian workers in Germany, with a slightly larger effect for workers with below-median wages, consistently with the fact marginal returnees are relatively young, thus likely at the beginning of their careers¹⁸. Furthermore, we find a stronger responsiveness of workers in small-medium-enterprises (10-19 employees) and in medium-sized firms (20-99). Last, the largest effects are estimated for individuals working in sectors such as IT and Communication and Finance, which is in line with the literature on migration responses to taxation (e.g. Kleven et al. (2014)).

6 Discussion and further extensions

Large emigration flows of young and highly educated individuals have characterized the recent history of many countries. While governments worry about reversing brain drain, few effective policies have been adopted. In this paper we investigate the effects of a unique policy to counteract emigration introduced by the Italian government in 2010. The reform granted large tax discounts to Italian migrants moving back to Italy, as long as they spent at least two years abroad, they had a college degree

¹⁸Only employed individuals are included.

and they were born on or after 1969. Exploiting the discontinuities in eligibility criteria in a Difference-in-Differences strategy on Italian administrative data on return migration flows, we find that return rates for the eligible group increase by about 50-60%. We then focus on Germany, the main destination country for Italian expatriates, and estimate similar effects on the probability that Italian workers return to Italy, as proxied by leaving the German social security data. Interestingly, we find that marginal returnees are mostly in the lowest half of the wage distribution in Germany, which is consistent with the fact that marginal returnees are often young college graduates at the onset of their careers. For both the Italian and the German analyses, results are robust to alternative definitions of the control group and sample restrictions. Overall, our findings suggest that tax-schemes-induced mobility can be a broader phenomenon than relocation of top earners in specific occupations (e.g. inventors in [Akcigit et al. \(2016\)](#) or football players in [Kleven et al. \(2013\)](#)) and may result in a substantial reallocation of human capital across sending and receiving regions.

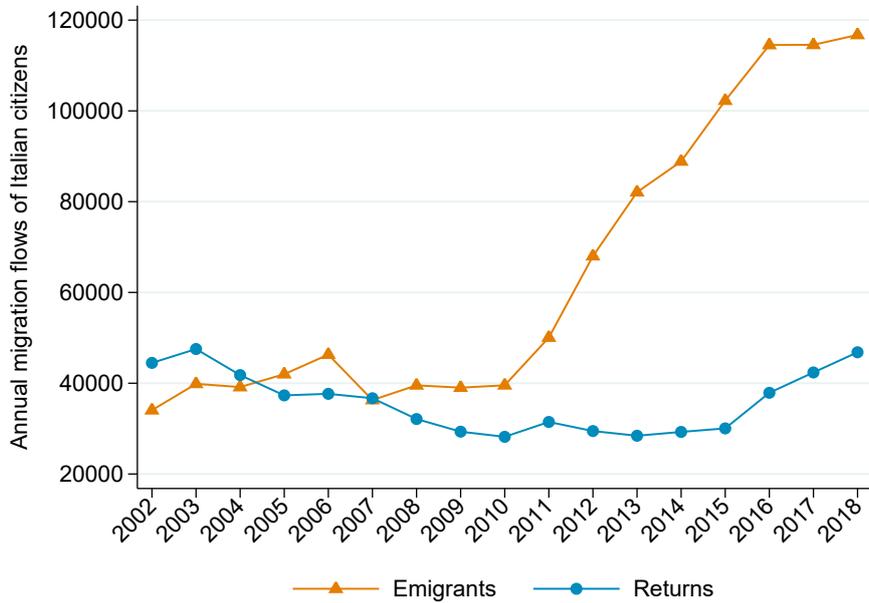
A few limitations of our study are worth highlighting and suggest some caution in interpreting our estimates. First, as we do not observe actual take-up, our estimates are intention-to-treat effects of eligibility for tax incentives on return migration. Second, we do not observe in the German data whether Italian citizens who leave the social security register actually return to Italy, although the similarity with the estimates using the Italian migration data suggest that the vast majority do. Last, and most important for policy implications, we do not observe in our data for how long eligible individuals who return stay in Italy, where they work and what they earn, which are all crucial piece of information to thoughtfully evaluate the effectiveness of tax incentives in reducing brain drain, and fostering economic growth also through return migration.

Moving forward, we plan to use Italian social security data to study the effects of tax incentives on labor market outcomes of returnees and of their co-workers. This data source would allow to explore fascinating extensions of this project. For instance, [Kleven et al. \(2014\)](#) found that gross wages of immigrants eligible for the Danish tax scheme suddenly *increased* after the scheme elapsed, suggesting that employers were retaining a fraction of the tax savings for the duration of the scheme.

In the Italian case, characterized by a remarkably high unemployment rate during the Great Recession, these wage effects may even be stronger and potentially limit the effectiveness of tax incentives in attenuating brain drain. Finally, having precise information about returnees' earnings would allow us to estimate the social welfare return on public resources spent (in terms of lower tax revenue) to attract back these individuals. As many countries have enacted or are enacting preferential tax schemes, these findings could have important implications for the policy debate about the effectiveness of policies aimed to mitigate brain drain, and potentially inform the design of preferential tax schemes in the future.

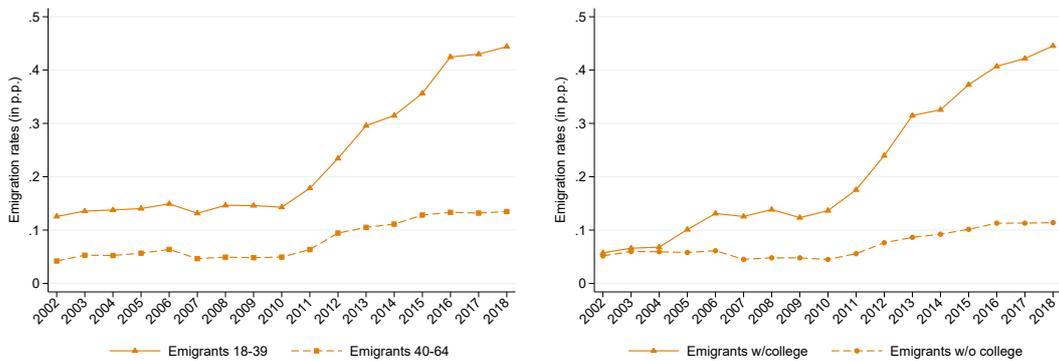
Figures

Figure 1: Annual emigration and immigration flows of Italians



Notes: annual outmigration (orange triangles) and immigration (blue circles) flows of Italian citizens. Source: authors' elaboration on Istat data.

Figure 2: Annual emigration flows of Italians, by age and education group



Notes: annual outmigration flows of Italians. Flows are in % of residents in each age/education group as of 2011 Census, multiplied by 100, therefore in percentage points. Source: authors' elaboration on Istat data.

Figure 3: Share living abroad among high-skilled nationals in 2010

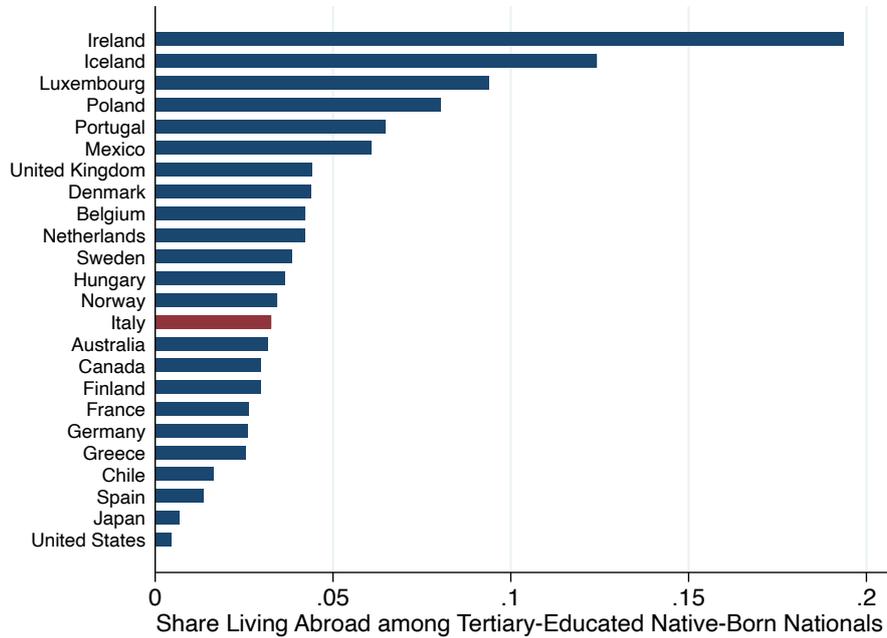
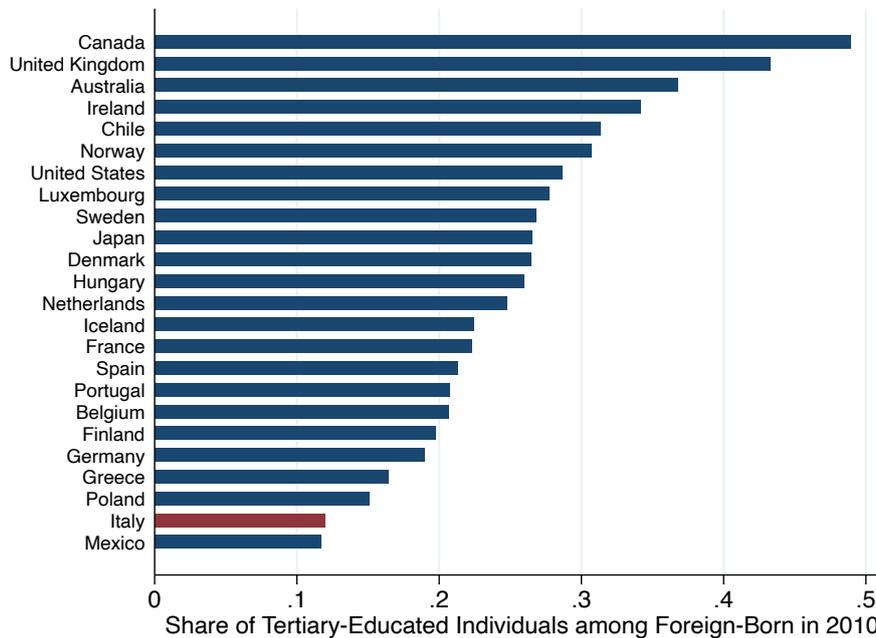


Figure 4: Share of high-skilled among immigrants in 2010



Notes: The top graph plots the share of tertiary-educated native-born nationals of each country relative living abroad relative to the sum of tertiary-educated native-born nationals both living abroad and in the country as of 2010. The bottom graph plots the share of tertiary-educated among foreign born individuals in each country as of 2010. Source: authors' elaboration on OECD DIOC data.

Figure 5: Emigration flows of Italians to the top-5 destination countries

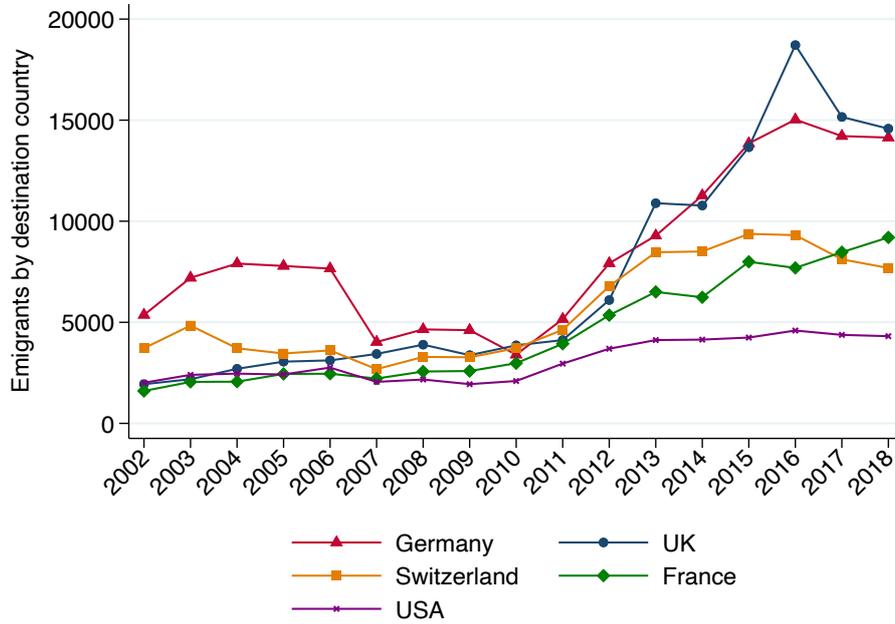
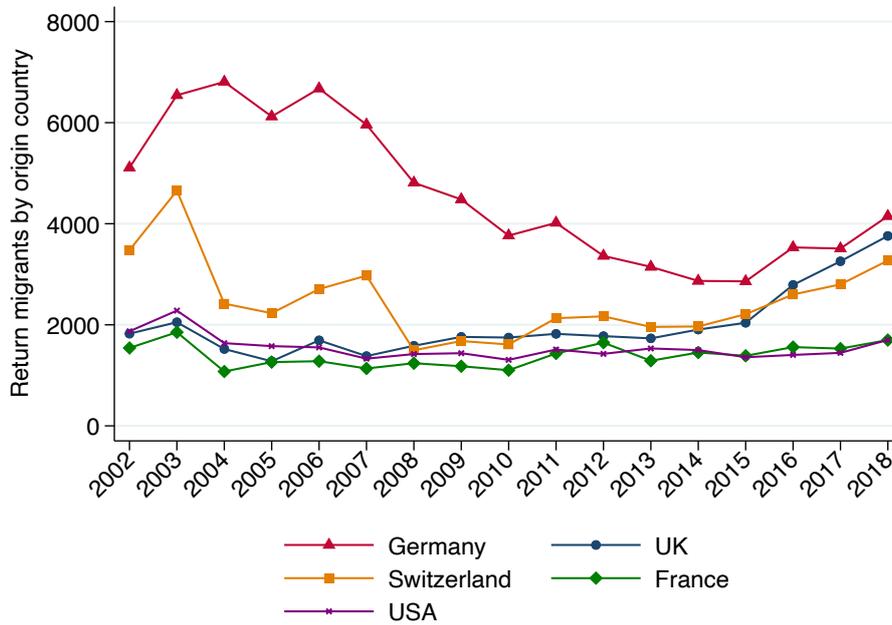


Figure 6: Return migration flows of Italians from the top-5 origin countries



Notes: The top graph plots the number of Italian citizens born in Italy migrating to each of the top-5 foreign countries of destination in each year, while the bottom graph plots the number of Italian citizens born in Italy moving to Italy from each of the top-5 foreign countries of origin.

Source: authors' elaboration on Istat data.

Figure 7: Timing, size and eligibility of tax incentives for high-skilled returnees

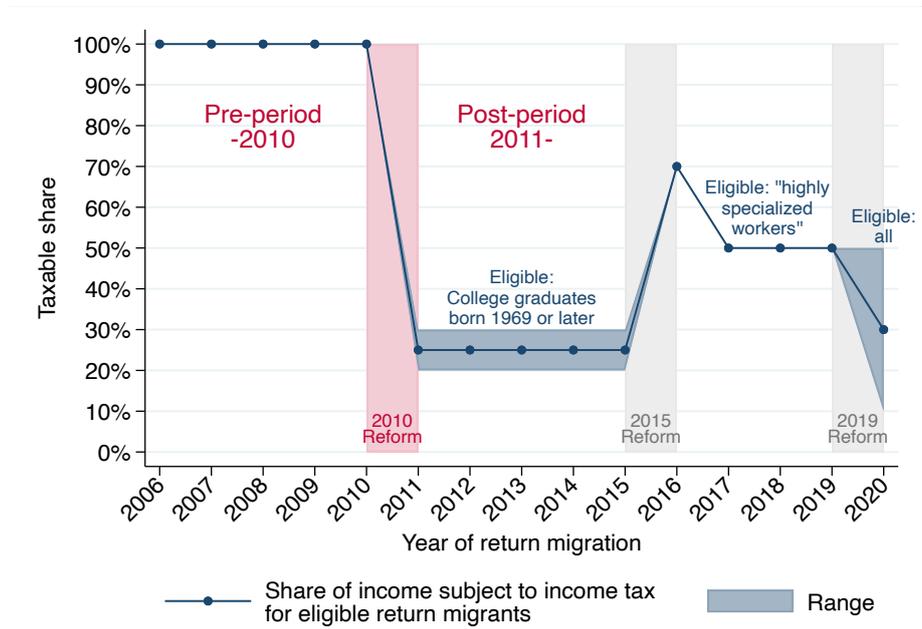
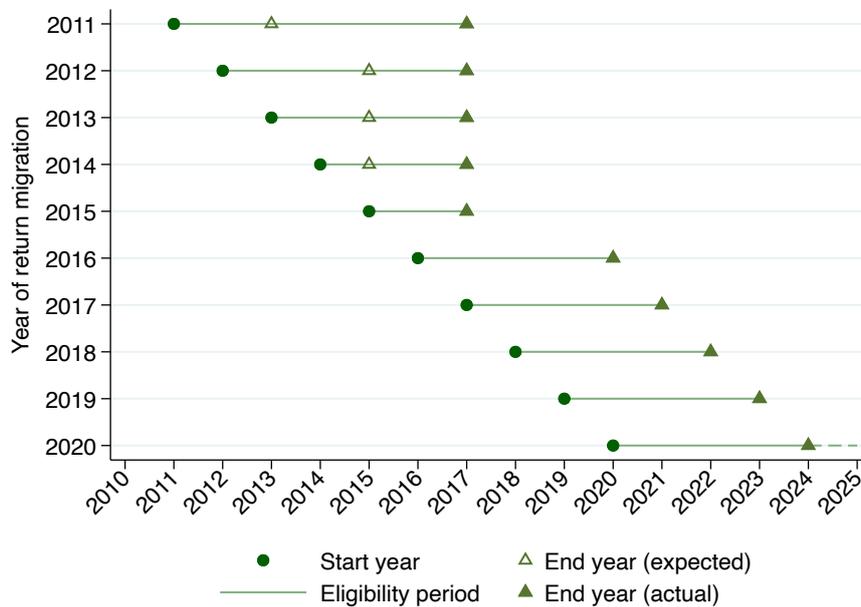
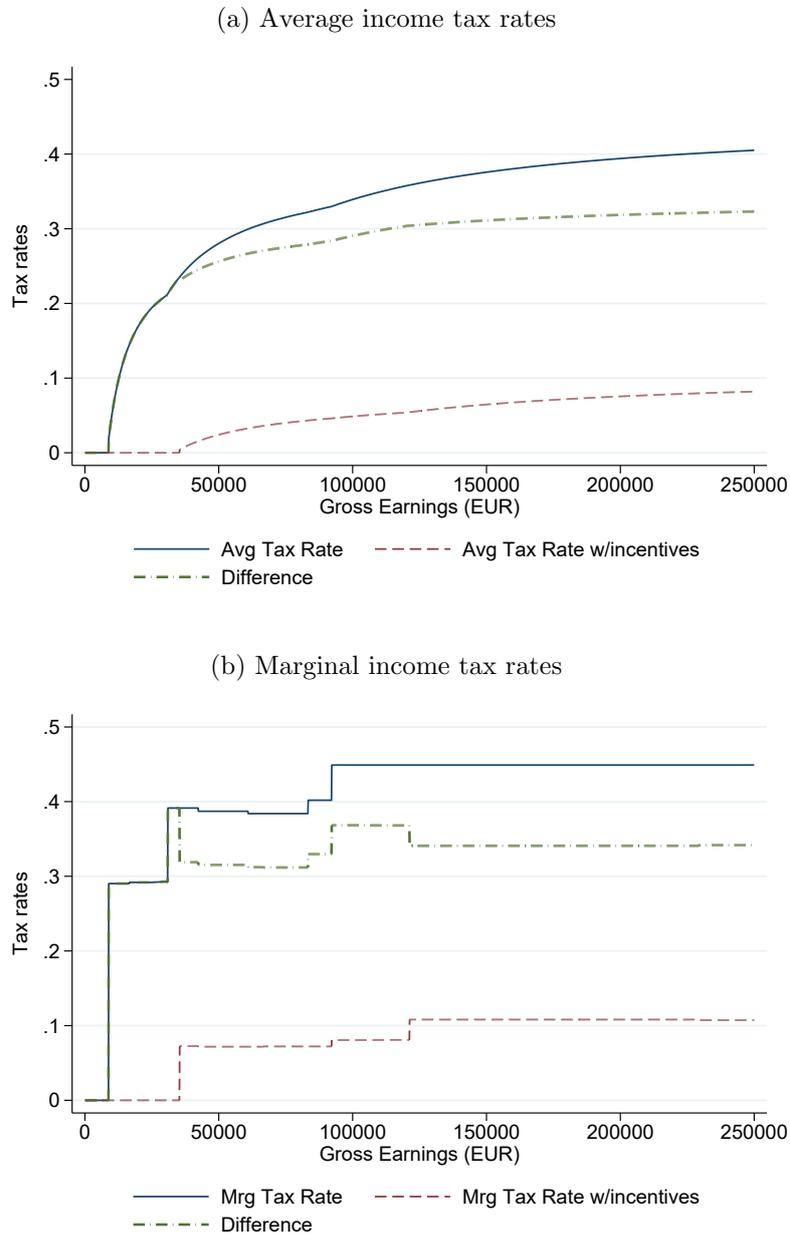


Figure 8: Duration of tax incentives for high-skilled returnees



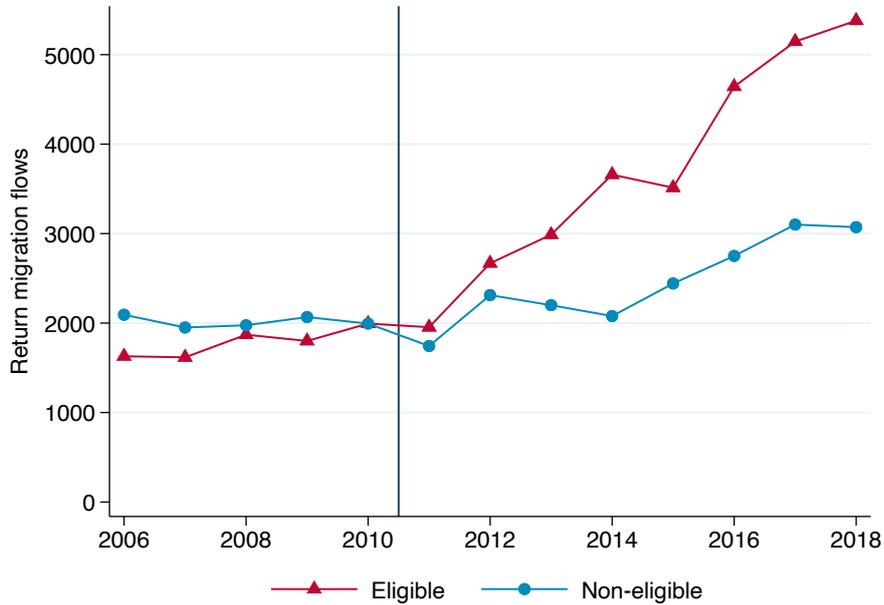
Notes: The top graph shows the share of labor income subject to income tax for return migrants eligible for the different tax schemes, depending on the year of return to Italy (on the horizontal axis). The bottom graph shows the start and the end year (as well as the expected end year, if different), depending on the year of return to Italy (on the vertical axis).

Figure 9: Average and marginal income tax rates with and without incentives of the 2010 tax scheme



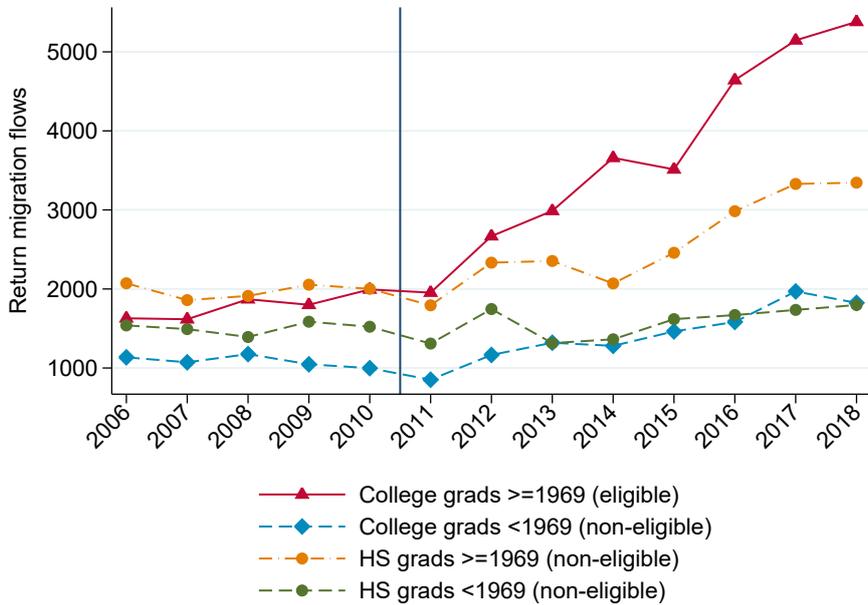
Notes: Figures 9a and 9b plot the average and the marginal income tax rates (respectively) based on the 2010 Italian tax schedule for an individual with no dependents (source: OECD Taxing Wages 2010). The fiscal incentive used is a 75% reduction in taxable income (Law 238/2010), i.e. an average between 70% (men) and 80% (women). For the tax rates with the tax incentives, gross earnings are assumed to be entirely deriving from employee labor income, self-employed labor income and/or business income.

Figure 10: Returns migration flows, by eligibility for tax incentives



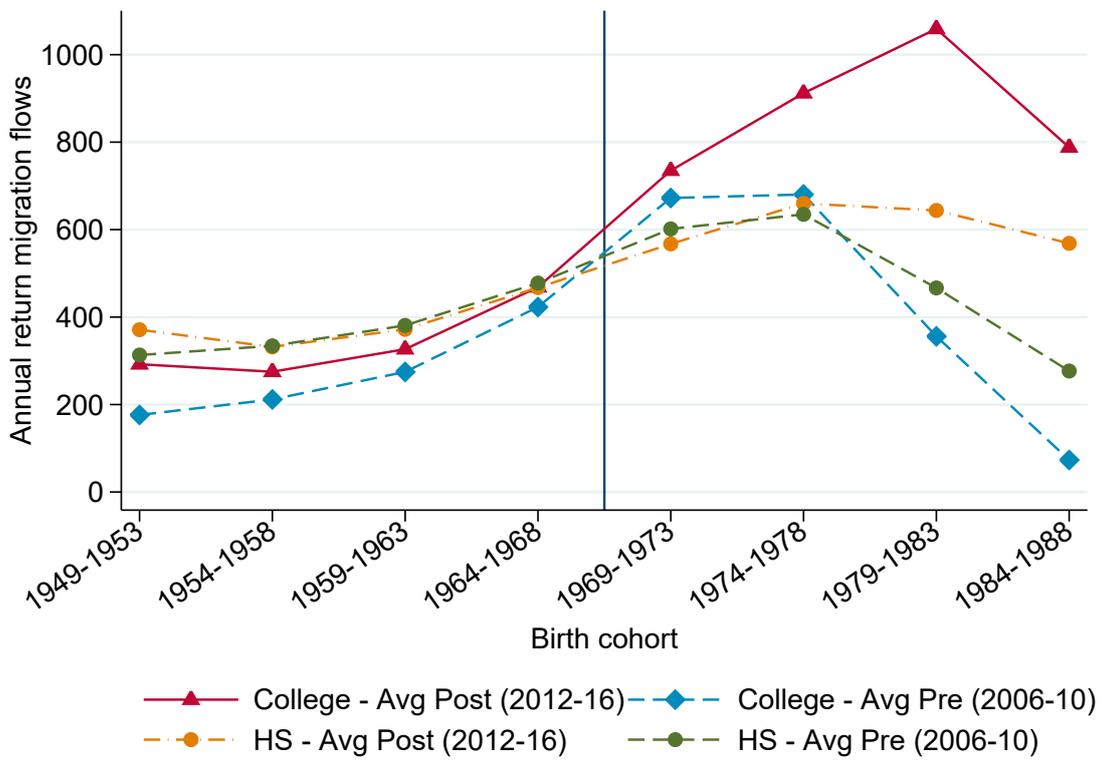
Notes: Non-eligible series is standardized to match the eligible in year 2010.

Figure 11: Returns migration flows, by education level and birth cohort group



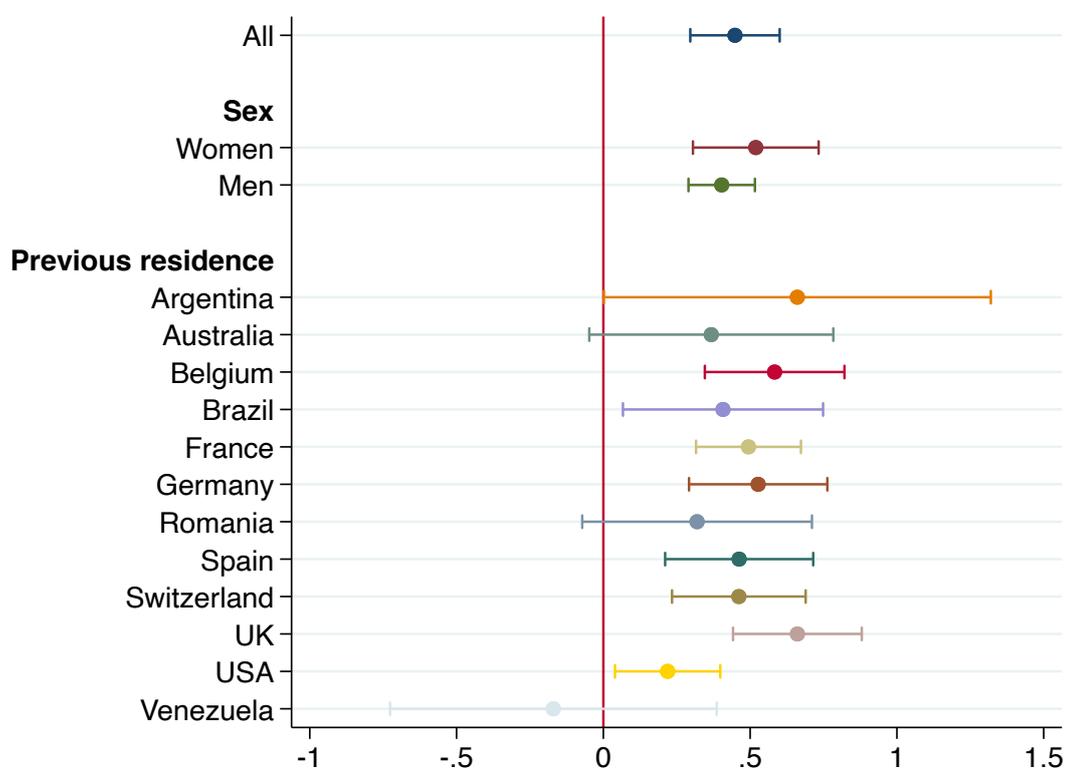
Notes: Istat data on the universe of Italian citizens who move their residence from abroad (AIRE) to Italy in each year. We exclude individuals born abroad as they may not ever been resident in Italy and thus may be ineligible, as well as individuals born before 1944 (thus older than 66 years old in 2010) and after 1989 (thus younger than 21 years old in 2010) who are likely not in the labor force.

Figure 12: Age distribution of returnees, separately by eligibility status and time period before (2006-2010) and after (2012-2016) the reform



Notes: The figure plots the average annual return migration flows, by birth cohort, for four combination of education and time period: college graduates returning after and before 2011, as well as high school graduates returning after and before 2011. Source: Istat data on the universe of Italian citizens who move their residence from abroad (AIRE) to Italy in each year. We exclude individuals born abroad as they may not ever been resident in Italy and thus may be ineligible.

Figure 13: Heterogeneous effects of tax incentives on returns



Notes: DiD estimates. Dependent variable is log returns. Bars denote 95% C.I.

Sample is all Italian citizens born in Italy, with at least a high school diploma, born between 1944 and 1989 and returning to Italy between 2006 and 2018. Source: authors' elaboration on Istat data.

Figure 14: Event study plot for the difference in probability of leaving the German social security registry, by eligibility for tax incentives in Italy

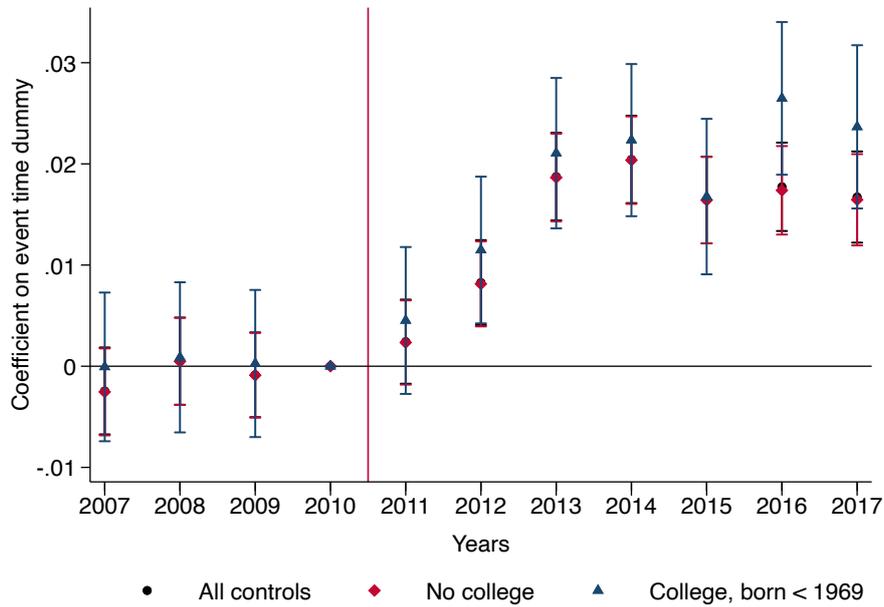
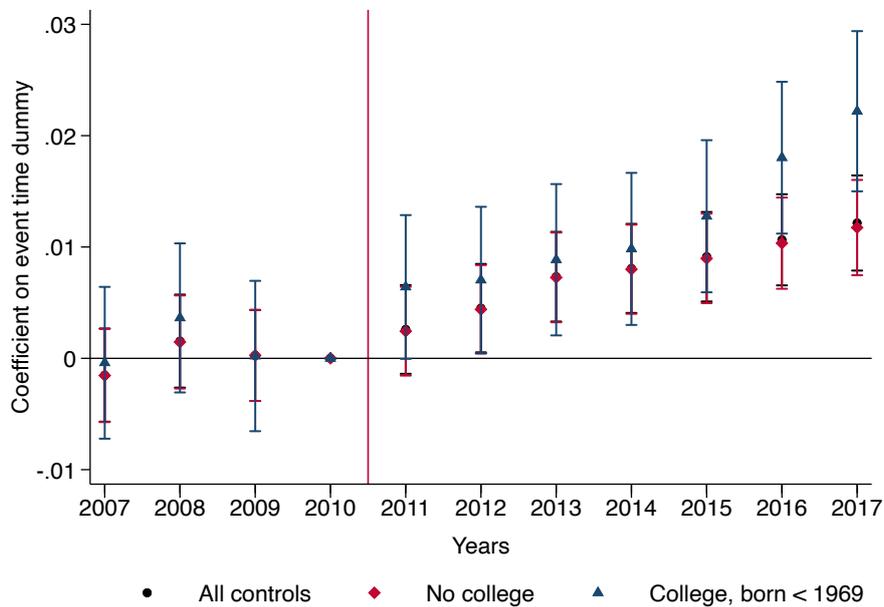
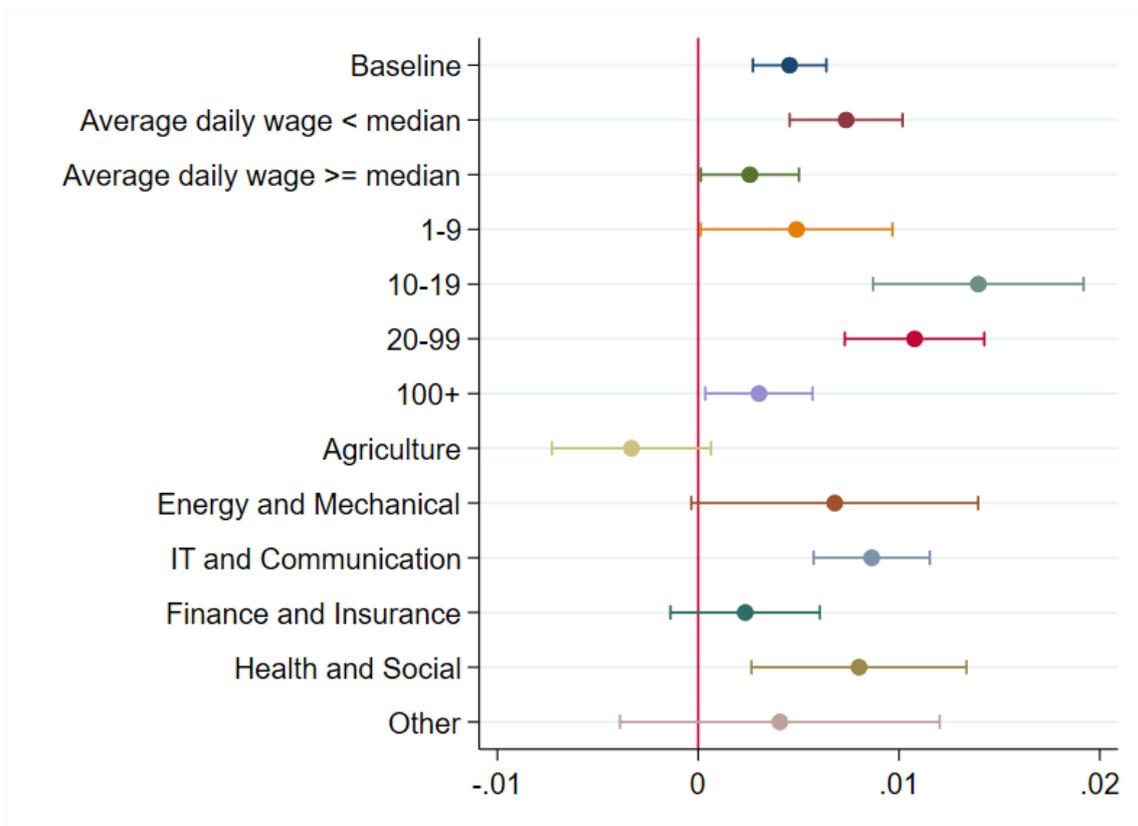


Figure 15: Event study plot for the difference in probability of leaving the German social registry, by eligibility for tax incentives in Italy - Employed only



Notes: Source: IEB data. Notes: the figures plot the difference in the probability to return, as proxied by exit of Italian citizens from the registry. The reference year is 2010. The treated group is all individuals born after 1969 with the maximum level of education achieved corresponding to university degree or higher. Control groups are Italians with high school diploma born after 1969 (“No college”), Italians with university degree born before 1969 (“College, born<1969”) and both (“All controls”). In the bottom figure, we only keep individuals whose last spell in the registry is an employment spell.

Figure 16: Heterogeneous effects of tax incentives on returns from Germany



Source: IEB data. Notes: the figure displays the point estimates and confidence intervals of the DiD coefficient for the baseline estimate and separately for subgroups of Italians, breakdown by whether their wage is below or above median, by firm size (1-9, 10-19, 20-99 and 100+ employees) and by sector. All regressions control for age at arrival, year and years in the register. Age of arrival is the age at first entry in the register. Sectors are aggregated from the German WZ08 Classification. Only Italians aged 23 or above and with a higher education or high school (or VET) degree are included. Bars denote 95% C.I.

Tables

Table 1: Characteristics of Italians in the German Social Security Data

Italians entered between 2000 and 2017, age at entry 25-64		
	Entered	Left
Female	39.41	39.21
Mean age	35.30	37.22
Degree	19.23	14.87
Mean duration in the register (years)	3.86	2.32
<hr/>		
Total individuals	208 156	104 652
Total establishments	44 155	40 294

Notes: the table displays basic characteristics of Italian workers in the private sector in Germany. Age of entry between 25 and 64. Nationality identified based on the mode value of the nationality variable. The restriction on age of entry aims at reducing the risk of considering also Italians born in Germany. Only individuals who entered from 2000 onwards are included. Mean age is the mean age at entry and the mean age at exit respectively. The total number of firms is based on the number of unique firm identifiers in which the Italian workers considered have worked for at least one spell. Source: our elaborations on the universe of Italians in the German social security data (IEB)

Table 2: DiD: effect of eligibility for tax incentives on return migration

	(1)	(2)	(3)
Control group	Log(Returns) All	Log(Returns) Coll<1969	Log(Returns) HS \geq 1969
Treated * Post	0.448*** (0.069)	0.446*** (0.070)	0.450*** (0.075)
Observations	26	26	26
R-squared	0.971	0.988	0.960
Avg Outcome	7.482	7.482	7.482
exp^n	1.566	1.562	1.568
Year FE	X	X	X

Notes: Observations: eligibility status (treated) by year (2006-2018) cells constructed from Istat data. The dependent variable is the log count of Italian citizens, born in Italy between 1949 and 1988 and with at least a high school diploma, moving to Italy from abroad in year t . The independent variables are “Treated”, a dummy equal to 1 if birth year is equal or greater than 1969 and education level is college, “Post”, a dummy equal to 1 for the post period years (2011 and after), and the interaction Treated * Post which captures the effect of eligibility for tax incentives. In Column 1 we include both high school graduates and college graduates born before 1969 in the control group, while in Columns 2 and 3 we only include college graduates born before 1969 and high school graduates born on or after 1969 respectively. “Average Outcome” refers to the treated group before 2011. Robust standard errors in parenthesis. * p \leq 0.10 ** p \leq 0.05 *** p \leq 0.01.

Table 3: DiD: effect of eligibility for tax incentives on return migration from Germany

	(1)	(2)	(3)
VARIABLES	Log(Returns) All	Log(Returns) Coll<1969	Log(Returns) HS \geq 1969
Treated * Post	0.527*** (0.107)	0.435*** (0.096)	0.571*** (0.126)
Observations	26	26	26
R-squared	0.930	0.957	0.831
Avg Outcome	5.544	5.544	5.544
exp^n	1.694	1.545	1.770
Year FE	X	X	X

Notes: Observations: eligibility status (treated) by year (2006-2018) cells constructed from Istat data. The dependent variable is the log count of Italian citizens, born in Italy between 1949 and 1988 and with at least a high school diploma, moving to Italy from Germany in year t . The independent variables are “Treated”, a dummy equal to 1 if birth year is equal or greater than 1969 and education level is college, “Post”, a dummy equal to 1 for the post period years (2011 and after), and the interaction Treated * Post which captures the effect of eligibility for tax incentives. In Column 1 we include both high school graduates and college graduates born before 1969 in the control group, while in Columns 2 and 3 we only include college graduates born before 1969 and high school graduates born on or after 1969 respectively. “Average Outcome” refers to the treated group before 2011. Robust standard errors in parenthesis. * p \leq 0.10 ** p \leq 0.05 *** p \leq 0.01.

Table 4: DiD effect on return migration rates

VARIABLES	(1)	(2)	(3)
	Return Rates All	Return Rates Coll<1969	Return Rates HS \geq 1969
Treated * Post	0.658*** (0.100)	0.550*** (0.084)	0.646*** (0.117)
Observations	26	26	26
R-squared	0.935	0.964	0.906
Avg Outcome	1.290	1.290	1.290
Year FE	X	X	X

Notes: Observations: eligibility status (treated) by year (2006-2018) cells constructed from Istat and OECD DIOC data. The dependent variable is the return migration rate of Italians abroad, and is equal to the count of Italian citizens, born in Italy between 1949 and 1983 and with at least a high school diploma, moving to Italy from abroad in year t , divided by the stock of Italian expatriates as of 2010. The independent variables are “Treated”, a dummy equal to 1 if birth year is equal or greater than 1969 and education level is college, “Post”, a dummy equal to 1 for the post period years (2011 and after), and the interaction Treated * Post which captures the effect of eligibility for tax incentives. In Column 1 we include both high school graduates and college graduates born before 1969 in the control group, while in Columns 2 and 3 we only include college graduates born before 1969 and high school graduates born on or after 1969 respectively. “Average Outcome” refers to the treated group before 2011. Robust standard errors in parenthesis. * p \leq 0.10 ** p \leq 0.05 *** p \leq 0.01.

Table 5: DiD effect on return migration rates from Germany

VARIABLES	(1)	(2)	(3)
	Return Rates All	Return Rates Coll<1969	Return Rates HS \geq 1969
Treated * Post	0.495** (0.168)	0.523*** (0.153)	0.639*** (0.174)
Observations	26	26	26
R-squared	0.941	0.930	0.892
Avg Outcome	1.685	1.685	1.685
Year FE	X	X	X

Notes: Observations: eligibility status (treated) by year (2006-2018) cells constructed from Istat and OECD DIOC data. The dependent variable is the return migration rate of Italians abroad, and is equal to the count of Italian citizens, born in Italy between 1949 and 1983 and with at least a high school diploma, moving to Italy from Germany in year t , divided by the stock of Italian expatriates in Germany as of 2010. The independent variables are “Treated”, a dummy equal to 1 if birth year is equal or greater than 1969 and education level is college, “Post”, a dummy equal to 1 for the post period years (2011 and after), and the interaction Treated * Post which captures the effect of eligibility for tax incentives. In Column 1 we include both high school graduates and college graduates born before 1969 in the control group, while in Columns 2 and 3 we only include college graduates born before 1969 and high school graduates born on or after 1969 respectively. “Average Outcome” refers to the treated group before 2011. Robust standard errors in parenthesis. * p \leq 0.10 ** p \leq 0.05 *** p \leq 0.01.

Table 6: Robustness checks: effects of eligibility on return migration

VARIABLES	(1) Log(Returns) Baseline DiD	(2) Log(Returns) EU + CH only	(3) Log(Returns) EU only	(4) Log(Returns) No 2016-18	(5) Log(Returns) No 2011	(6) Log(Returns) Triple DiD
Treated * Post	0.448*** (0.069)	0.544*** (0.079)	0.569*** (0.083)	0.385*** (0.084)	0.478*** (0.068)	0.447*** (0.118)
Observations	26	26	26	20	24	208
R-squared	0.971	0.958	0.944	0.957	0.977	0.955
Avg Outcome	7.482	7.039	6.952	7.482	7.482	5.847
<i>expⁿ</i>	1.566	1.723	1.767	1.469	1.613	1.564
Year FE	X	X	X	X	X	X
Birth cohort FE						X
Educ FE						X
Cohort-by-Educ FE						X
Cohort-by-Year FE						X
Educ-by-Year FE						X

Notes: The dependent variable is the log count of Italian citizens, born in Italy between 1949 and 1988 and with at least a high school diploma, moving to Italy from abroad in year t . The independent variables are “Treated”, a dummy equal to 1 if birth year is equal or greater than 1969 and education level is college, “Post”, a dummy equal to 1 for the post period years (2011 and after), and the interaction Treated * Post which captures the effect of eligibility for tax incentives. In Columns 1 to 3 (DiD), observations are eligibility status (treated) by year cells, 2006-2018 in Column 1, 2006-2015 in Column 2 and 2006-2018 except 2011 in Column 3. In Column 4 (Triple DiD), observations are education (high school and college) by birth cohort (8 five-year groups from 1949 to 1988) by by year (2006-2018) cells. Fixed Effects included: Year, Birth Cohort, Education, Cohort-by-Educ, Cohort-by-Year, Educ-by-Year. Robust standard errors in parenthesis. “Average Outcome” refers to the treated group before 2011. Robust standard errors in parenthesis.* p \leq 0.10

** p \leq 0.05 *** p \leq 0.01.

Table 7: DiD effect of eligibility on the probability of leaving the German social security registry

	(1) All controls	(2) No college	(3) CollegeBorn<1969
<i>Panel A: All workers</i>			
Treated*Post	0.011 *** [0.0009]	0.010 *** [0.0010]	0.012 *** [0.0016]
Mean outcome (treated at t = 0)	0.034	0.034	0.034
Observations	1,851,074	1,788,001	272,301
Individuals	221,278	213,442	42,578
R2	0.0136	0.0138	0.0201
<i>Panel B: Only employed</i>			
Treated*Post	0.005 *** [0.0009]	0.004 *** [0.0009]	0.006*** [0.0015]
Mean outcome (treated at t = 0)	0.028	0.028	0.028
Observations	1,587,216	1,532,365	239,818
Individuals	209,221	201,982	39,522
R2	0.0114	0.0115	0.0151

Notes: Source: IEB. Notes: the outcome variable is the probability of leaving the register compared to being still in the register at $t+1$. All migrants are included as long as their highest reported educational level is either high school diploma (and VET studies) or university degree and if the mode of the nationality variable is "Italian". Controls include gender, age at entry in the register, birth cohort and year fixed effects. Standard errors are clustered at the individual level. * p < 0.10 ** p < 0.05 *** p < 0.01.

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Chapter 4

Home Country Conditions, Return Intentions and Labor Market Outcomes: Evidence from Terrorist Attacks Worldwide.

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Abstract

Migrations are often temporary and the decision to remain in one country is an important determinant of immigrants' labor market behaviors, human capital investments and their socio-economic integration in host countries. In this paper, we investigate whether changes in the socio-political conditions in the home country affect immigrants' return intentions and labor market integration. We combine administrative and survey data with precise information on terrorist attacks world wide. Exploiting the quasi-random occurrence of terrorist attacks relative to interview dates and unemployment registrations, we obtain two key results. First, immigrants interviewed after terrorist attacks are 3.5 percentage points more likely to remain in Germany permanently. Second, immigrants who enter unemployment when terrorist attacks hit their home countries re-enter employment faster, compared to immigrants who enter unemployment in quiet times. Our paper is the first to show that shocks to home country conditions affect not only return intentions but also the labor market behavior of immigrants abroad.

Key Words: terrorism, return migration, unemployment

JEL Codes: J16, J24, J61, J64

1 Introduction

Most migration decisions are not permanent. Immigrants revise their return decision as a result of changes in their personal circumstances and in aggregate conditions in both home and host country. Revisions to the intended length of stay may lead to subsequent changes in the socio-economic behaviour of migrants in the host country. Previous research has shown that migrants who plan to stay longer are more likely to invest in host country human capital which can lead to steeper earnings and career paths (Akay et al., 2020; Bratsberg et al., 2002; Cortes, 2004; Dustmann, 1993, 1999).

Individual characteristics and the length of residency have been shown to matter greatly for migrants' intention to remain, naturalization rates, self-identification and long-term labour market outcomes (Akay et al., 2020; Bijwaard and Wahba, 2014; de Coulon et al., 2016; Dustmann, 1993, 1997; Gibson and McKenzie, 2011; Nekby, 2006). However, the socio-political context at the country of origin and destination can also act as push and pull factors which affect migrants intentions to return migrate. While some recent studies have looked at how changes in socio-political conditions in the host country increase return intentions among migrants (Steinhardt, 2018; de Coulon et al., 2016) and worsen their socio-economic integration (Gould and Klor, 2016; Elsayed and De Grip, 2018; Steinhardt, 2018; Schilling and Stillman, 2021), there is little empirical evidence on how changes in the socio-political conditions in the home country affect these outcomes.

In this study we address this gap in the literature and investigate whether shocks to socio-political conditions in the home country affect return intentions, and in turn the labor market behavior of immigrants in Germany. The underlying mechanism being that negative socio-political events in the home country work as shocks to migrants' location preferences and increase the attractiveness of the destination country relative to the home country. Our results show that a negative shock to the socio-political conditions in the home country increases migrants' intention to remain in Germany permanently, which translates into a higher job search effort among unemployed immigrants.

In the empirical analysis we proxy changes in socio-political conditions in the home country with the occurrence of terrorist attacks. Data on terrorist attacks come

from the Global Terror Database (GTD), a large dataset containing information on almost 200000 terrorist events worldwide and from 1970 to 2017. Events are recorded daily and the geographical location where the events took place is highly precise. Additionally, the dataset includes events' characteristics such as the number of killed and wounded, which allows us to investigate the effect of both occurrence and intensity of terrorist events.

In the first part of the analysis we combine the GTD data with the German Socio-Economic Panel (GSOEP) and investigate the effect of terrorist events in the home country on migrants' intention to remain in Germany. The GSOEP is a large-scale survey, representative of the German population. It has been run yearly since 1984 and includes a wide variety of individual-level information. Crucial for our analysis, it also collects information on the nationality, the year of migration, the intention to stay and the level of German knowledge among immigrants in Germany. The identification strategy in this section relies on the quasi-random occurrence of the date of the event at origin relative to the timing of the GSOEP interviews and the characteristics of the respondent being interviewed. Our main results show that migrants interviewed right after terrorist events are 3.5 percentage points more likely to declare that they want to stay in Germany permanently. The effect is particularly strong among immigrants who were less integrated before the terrorist event (e.g., scarce German knowledge) and has a more immediate effect if the event occurs in countries with higher internet penetration.

As one of the crucial identifying assumption is that the occurrence of terrorist events in the home countries did not interfere with the implementation of the survey, we provide a series of balance tests as evidence in favor of our assumption. We also show that the main results are not driven by specific countries or survey years and that when we assign placebo events, there are no effects on the intention to stay.

In the second part of the analysis, we analyse the effect of terrorism on measurable labour market outcomes. More specifically, we look at how terrorist events in the home country affect the job search behaviour and the uptake of re-training programs among unemployed immigrants in Germany. We focus on these outcomes as they are likely to have a more immediate reaction to a shock in return intentions - driven by a terrorist event. To accurately measure time to employment (e.g. proxy for job

search intensity) and the participation in re-training programs we rely on German administrative data (IEB), using the 10% of the immigrant population in the social security records between 2000 and 2018. In our empirical strategy we compare the labor market outcomes of immigrants entering unemployment when terrorist events occur in their home countries to those of immigrants that entered unemployment in times of stable home country conditions.

Our results show that when immigrants enter unemployment during terrorist attacks, they re-enter employment 16 days faster and that the effect is larger the higher the intensity of the attack. These results are robust to placebo treatment assignments and alternative definitions of terrorism. Moreover, we combine our data with information on firms' mass-layoffs and bankruptcies and restrict our analysis to only displaced immigrants, to reduce the bias from self-selection into unemployment. Results hardly change. We argue that our findings are driven by a sense of "fear" that a negative shock in the home country creates among unemployed immigrants specifically. This fear is driven by the idea of being potentially obliged to return to the country of origin due to unsustainable economic conditions in the host country. Hence, home country terrorist events seem to create an incentive for unemployed immigrants to find a job faster as a way to retain their visas or to economically ensure a stay in the host country, for instance.

We contribute to the literature in three ways. First, we provide empirical evidence on the link between return intentions and socio-political conditions in the home country. Previous literature has shown that economic conditions in the home country matter for the well-being of immigrants ([Akay et al., 2017](#)) abroad and that they may determine both migration flows and the size of remittances ([Gröger, 2021](#)). However, the link between home country socio-political conditions and return intentions has only been theoretically hypothesized ([Dustmann and Görlach, 2016](#)). Moreover, the only empirical evidence on violence and return intentions studies contexts in which violence increases in the host country ([Steinhardt, 2018](#)). We are the first to show that violence in the home country also affects return decisions.

Second, we contribute to the literature on the effects of external shocks on the labor market integration of immigrants. Previous studies have shown that terrorism in the host country affects immigrants' integration. For example, [Gould and Klor \(2016\)](#)

show that the 9/11 attacks had long-lasting effects on the integration of Muslim immigrants, while [Brodeur and Wright \(2019\)](#) show that the same events also reduced asylum approval rates. Closest to our paper is [Steinhardt \(2018\)](#) who finds that xenophobic violence reduces immigrants' investments in language skills. We show that terrorist events at home do not only affect return intentions, but also the labor market behavior of immigrants. While we cannot directly link the effect of terror on return intentions to its effect immigrants' labor market behavior, we show that terror events that create a plausible shock to return intentions also have an effect on the search behavior of immigrants.

Third, despite using terrorism as a proxy for socio-political turmoil and violence in the home country, our paper is closely related to the literature on terrorism and its effect on well-being and mental health. A number of studies find that terrorism affects political opinions and voting behaviors ([Peri et al., 2020](#)), reduces the well-being of individuals ([Akay et al., 2020](#); [Clark et al., 2020](#)), and of immigrants from affected countries in particular ([Sønderskov et al., 2021](#); [Keita and Schewe, 2021](#)). Using comparable research designs, we show that terrorism also affects other outcomes, such as return intentions.

The rest of the paper is organized as follows. Section 2 describes the analysis on terror and return intentions. Section 3 focuses on the analysis of terror and labor market behavior. Section 4 concludes.

2 Terrorism and Return Intentions

A large share of migration episodes is temporary (OECD 2008), and around 20 to 50 percent of immigrants leaves the host country after 5 years since arrival. Temporary migrations are becoming more frequent in recent decades as freedom of movement increases, the economy is more globalized and travel costs sunk. While many empirical studies have treated temporary migration as a selection issue that might bias the estimation of immigrants' assimilation profiles (see e.g., [Lubotsky, 2007](#)), recent research has investigated the importance of return migration - and return intentions - on the socio-economic behavior of immigrants ([Dustmann and Görlach, 2016](#)). Intentions to leave or stay in the host country may have an effect, for

example, on investments in human capital formation (Adda et al., 2021) or savings behavior (Bauer and Sinning, 2011).

Additionally, Dustmann and Mestres (2010) showed that return intentions have an effect on migrants' remittance behaviors, which in turn has a large impact on the origin country's economy.

Given the importance of temporary migrations, several studies have analyzed their individual determinants (see e.g., Bijwaard and Wahba, 2014; Dustmann and Görlach, 2016). Far less studied are the country-level determinants of return migrations and those existing focus on macro-economic conditions (Akay et al., 2017; Bijwaard et al., 2014). In this paper, we investigate whether socio-political conditions in the home country affect return intentions, and in turn the labor market integration of immigrants.

As we proxy socio-political conditions with terrorist events, we combine research on return migration with studies on the effects of terrorism on individual outcomes. In particular, a recent strand of research has shown that terrorist events casually affect the short-term well being of individuals (Clark et al., 2020; Akay et al., 2020; Keita and Schewe, 2021; Sønderskov et al., 2021). For example, Akay et al. (2020) show that the level of terror in the world² has a negative effect on the well-being, and this effect is present contemporaneously in different countries. Keita and Schewe (2021) focus specifically on immigrants and find a negative effect of terror in the home country on the well-being of refugees in Germany³.

Combining these two strands of literature, in this part of the analysis we test whether a negative socio-political (e.g., a terrorist event) has a positive effect on immigrants intention to stay in Germany. We hypothesize the following mechanism: a negative socio-political event in the home country works as a shock to immigrants' location preferences, increasing the attractiveness of the host country relative to the home country and therefore increasing the desire to remain permanently in the host country or to delay the timing of return migration.

²Proxied by the total number of casualties in a day

³Other studies have shown that deteriorating macroeconomic conditions in the home country, such as an increase in the GDP (relative deprivation), currency valuation or natural disaster (Akay et al., 2017; Bijwaard et al., 2014) can also have an effect on migrants well-being in the host country

2.1 Data

German Socio-Economic Panel We use the full dataset from the German Socio-Economic Panel (GSOEP) from 1984 to 2018. The GSOEP is a large-scale yearly household survey that is representative of the German population⁴. The dataset contains both individual and family information on a wide set of topics, from education, to work-life, to consumption, to more behavioral and attitudinal characteristics. Crucial to our analysis, a large number of immigrants is interviewed each year. If they have a migration background, respondents are asked migration-specific questions, such as the country of origin, the presence of family abroad, their German knowledge and their return intentions. The GSOEP has been widely used to study immigrants in the German society, and specifically to study return migration intentions (see e.g. [Dustmann and Görlach, 2016](#); [Bauer and Sinning, 2011](#)).

Figure 1 plots the share of immigrants that intend to remain in Germany permanently for the largest nationality groups in the GSOEP. While Eastern European immigrants (some of which are ethnic Germans) tend to have stable return migration intentions, for other nationality groups the share of immigrants who want to settle permanently has increased over time⁵.

In Table 1, we show descriptive statistics of the migration population in the GSOEP. A very high share of the migrants in Germany over the period under analysis have only lower secondary education or below. While the mean of the full-time employed over the 1984-2018 period is only 0.41, these results are driven by the large inflows of refugees Germany has hosted over the years and by the low labour force participation among female migrants. Refugee employment over the first two to three years after migration is relatively low but it then catches up with the rest of the migrant population. Finally, migrants seem to be satisfied with their lives in Germany (e.g., life satisfaction is measured on a scale from 0 to 10) and most want to remain in Germany for many years.

Global Terror Database The Global Terrorism Database (GTD) is an open-source database that provides detailed information on terrorist incidents worldwide ([LaFree](#)

⁴For a complete description of the data, please refer to [Goebel et al. \(2019\)](#)

⁵Part of the increase in intentions to stay may be due to compositional changes and panel attrition. In Appendix C.2.1 we show the share of migrants in the GSOEP over time and discuss the different migration waves to Germany in more details.

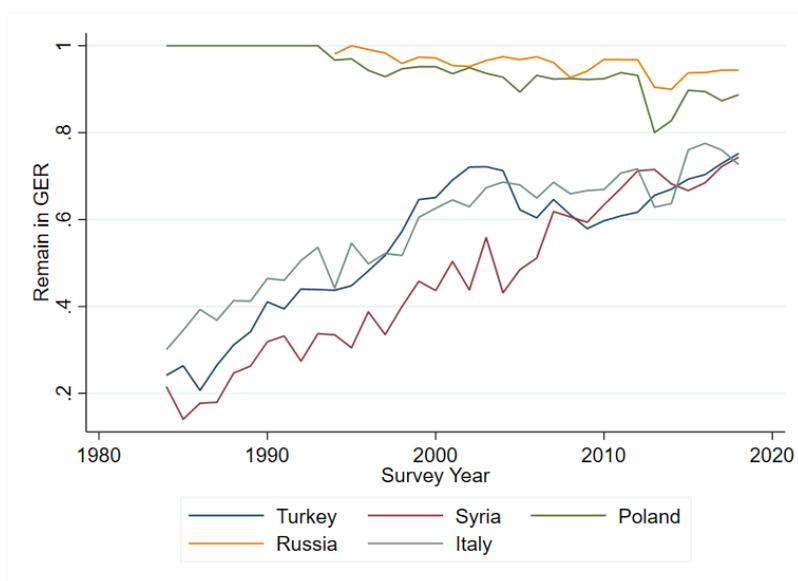
Table 1: Summary characteristics of the migrant population in the GSOEP data

	Mean	SD	Median
Female	0.498	0.500	0.000
Age	41.94	14.02	41.00
Years since mig.	17.29	11.52	16.00
Low Sec. or bellow	0.425	0.494	0.000
Upper Secondary	0.320	0.466	0.000
Post-Secondary	0.110	0.312	0.000
Higher education	0.145	0.352	0.000
Marital status	0.717	0.450	1.000
No children	0.532	0.499	1.000
One child	0.333	0.471	0.000
Two or more children	0.135	0.342	0.000
Full-time employ.	0.410	0.492	0.000
Part-time employ.	0.093	0.290	0.000
Other employ.	0.065	0.247	0.000
Not employed	0.432	0.495	0.000
Household Income	2171	1442	1943
Life Satisfaction	7.167	1.895	7.000
Remain GER many yrs	0.974	0.159	1.000
Remain GER perman.	0.710	0.454	1.000
Observations	99,807		

Notes: Table 1 reports the main characteristics of the full sample of immigrants in the GSOEP data (1984-2019). For each variable, we report the mean, standard deviation and median value. The last row reports the total number of immigrants.

Source: GSOEP

Figure 1: Remain in Germany permanently, main groups



Notes: Figure 1 displays the share of immigrants that intend to remain in Germany permanently. Shares are computed for each survey year (from 1984 to 2019) only for the 5 largest nationality groups.
Source: GSOEP

and Dugan, 2007). Data are collected using both human and machine intelligence. First, millions of articles from newspapers worldwide are processed daily to find and document all terrorist events. Natural language processing, named entity extraction, and machine learning models are used to identify and organize news articles that include information about terrorist attacks. The GTD team has developed a proprietary data management system that allows analysts to identify unique attacks, record the details of each event, and update records for previously recorded events as new information becomes available (The Global Terrorism Database, 2019). This process allows the GTD to collect many details about reported terrorist attacks, including the precise location, property destruction value, and the number of wounded and killed people. It also includes information about the type, perpetrator, target, and purpose of the attack for some events (LaFree and Dugan, 2007).

In Figure 2 and Table 2 we present descriptive statistics on the terror events from the GTD database. Figure 2 shows monthly trends in terror events and in the total number of affected individuals (i.e. the sum of killed and wounded) for the four non-EU countries with the largest immigrant population in Germany (Turkey, Syria, Poland and Kazakhstan). The number of events and affected individuals strongly

varies over time and across countries. For example, Syria experienced a spike in terror events in the last five years, while these are more evenly distributed for the other countries. Additionally, while Poland and Kazakhstan have only few scattered events, Turkey has experienced frequent events from the 1970s up to nowadays.

In Table 2, we report descriptive statistics on terror events occurring outside of Germany. 88.2% of all events are coded as successful⁶, the average number of killed per terror event is 2.4, while the average number of wounded is 3.2, the majority of terror events targets civilians and political actors. Finally, almost half of the terror events took place in the last decade. This pattern might be driven by both the increased violence in many regions of the world (e.g. Middle East, North Africa), and the higher accuracy in the registration of events with the diffusion of online news. Given the large number of monthly events in many countries, we define one month as the treatment month ($t = 0$) if the number of terror events in that month are higher than the three-years average number of monthly terror events. Contrary to previous papers that consider the absolute number of casualties (see e.g., Akay et al., 2020; Keita and Schewe, 2021; Sønderskov et al., 2021), we introduce a relative measure of terror that takes into account country-specific periods of high and low incidence of terror events. This measure is based on the idea that individuals coming from countries with a high number of terrorist events in the recent past have a different reference point when compared to individuals coming from countries which have very rare terrorist attacks. One terrorist event in a country such as France in 2016 is likely to create a bigger shock and a larger reaction among french migrants abroad than one terrorist event in Syria, for instance, which was experiencing a period of intensive turmoil in 2016.

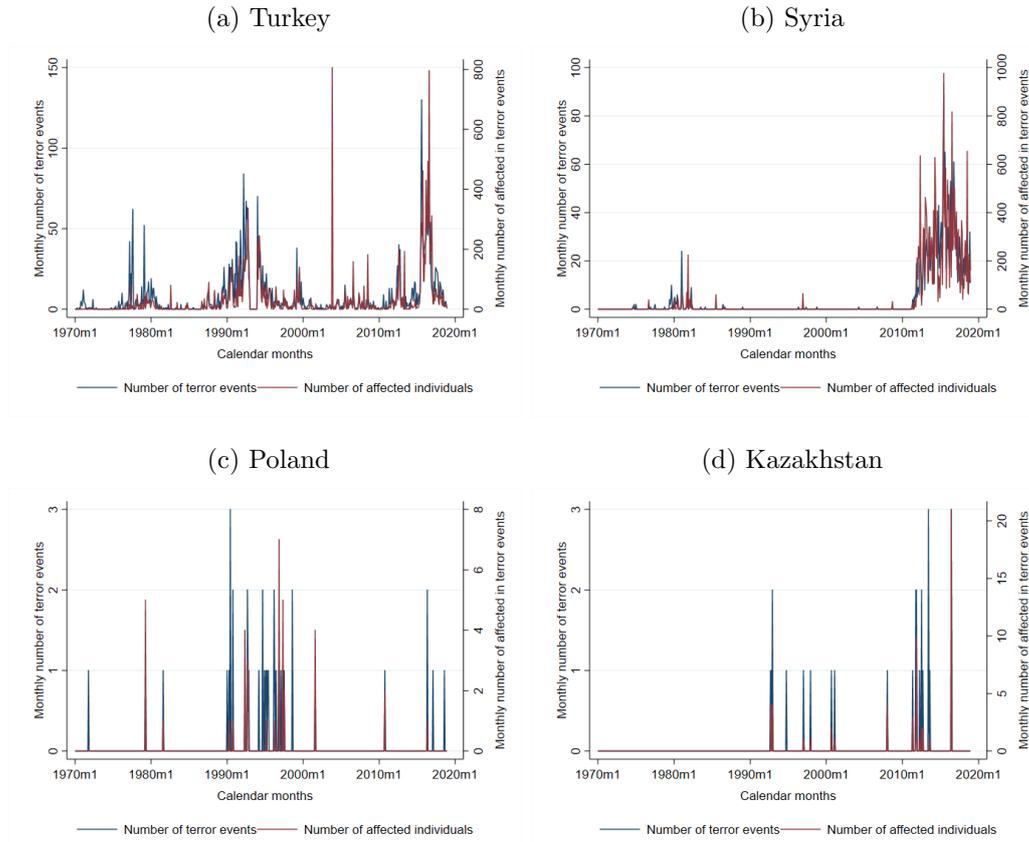
2.2 Empirical Strategy

To estimate the effects of terrorist attacks on the intentions to remain, we exploit the variation induced by the timing of interviews in the SOEP and the timing of terror events in the home country⁷. We estimate the following model:

⁶The definition of a successful/failed attack - provided in the GTD database - depends on the type of attack. For instance, an assassination is considered successful if the target is killed, while an explosion is considered successful if the explosive device detonates (Brodeur and Wright, 2019)

⁷This design has been recently used to study also the effect of terrorism on well-being (Akay et al., 2020; Clark et al., 2020) and political opinions (Peri et al., 2020), as well as the effect of

Figure 2: Monthly terror events and affected individuals for selected countries



Notes: Figure 2 displays the distribution of terrorist events (blue line) and affected individuals (red line) over time for a selected group of countries. The four countries are the largest non-EU nationality groups in the GSOEP. We do not include Russians as the vast majority of immigrants of Russian nationality in the GSOEP are ethnic Germans. The number of affected individuals is the sum of killed and wounded from terror events in each month and country. The left y axis refers to the number of monthly terror events, while the right y axis to the monthly number of affected individuals. The time window is 1970-2017. Source: Global Terrorism Database (GTD).

$$I_{i,t} = \sum_{t=-P}^T \beta_t Time_{i,j} + \gamma X_i + \delta_j + \epsilon_{i,j} \quad (6)$$

where $I_{i,t}$ is a measure of return intentions or perceived integration in the German society. $Time_{i,j}$'s are dummies identifying periods around the event where $t = 1$ for individuals interviewed 1 month after the event, $t = 2$ for those interviewed after 2 months. Similarly, $t = -1$ for those interviewed 1 month before the event,

football victories in international competitions on national identity sentiments (Depetris-Chauvin et al., 2020)

Table 2: Summary characteristics of terror events

% Successful	88.2 %
Average Killed	2.4
Average Wounded	3.2
Target type	
<i>Civilians and business</i>	40.7 %
<i>Political actors and military</i>	43.8 %
<i>Infrastructures</i>	15.5 %
Decades	
<i>1970-1980</i>	6.6 %
<i>1981-1990</i>	16.9 %
<i>1991-2000</i>	13.9 %
<i>2001-2010</i>	14.7 %
<i>2011-2020</i>	47.9 %
Total events	191464

Notes: Table 2 reports the main characteristics of terror events from the Global Terrorism Database. The target type is an aggregated version of the original variable in the dataset which includes a wider variety of targets. Statistics are computed on the total of daily events that occurred worldwide from 1970 to 2017. The total number of daily events is recorded in the last column.

Source: Global Terrorism Database (GTD)

and so on. The coefficients β_1, \dots, β_T identify dynamic treatment effects, $t = 0$ is the baseline omitted period. δ_j are terror event-specific fixed effects defined at the month-year-country of origin level, X is a set of individual covariates. To precisely estimate the effects of terror events, in our main specification we include only immigrants interviewed within a 12 weeks bandwidth from the occurrence of the event.

The inclusion of terror event-specific fixed effects allows us to compare outcomes for immigrants from the same country of origin that are interviewed in the same year right before or right after the event. The estimated coefficient is then an average of the effects across countries of origin and terrorist events.

To summarize the average treatment effect over all periods we also estimate:

$$I_{i,t} = \beta \text{Error}_{i,j} + \gamma X_i + \delta_j + \epsilon_{i,j} \quad (7)$$

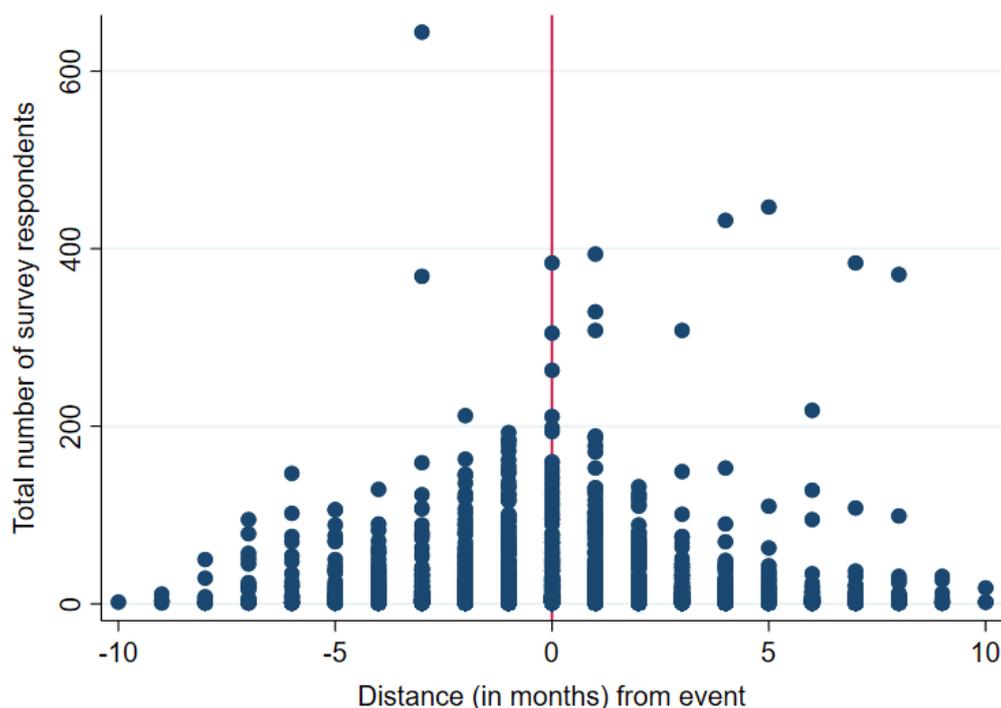
where time dummies are substituted with the indicator Error_t which takes value

of 1 if a respondent from country of origin o is interviewed in a month m and year y within three periods (i.e., months) t after the terror event, and 0 if a respondent is interviewed within three periods before the event.

Our identification strategy relies on the quasi-random occurrence of terror events relative to the precise timing immigrants are interviewed. Therefore, our identifying assumption is that the occurrence of terror events in the home countries did not interfere with the implementation of the survey. While it is unlikely that the organization of the survey changes in response to terror events, it may happen that immigrants who are more attached to their home countries refuse to be interviewed after the event. This non-random selection may bias upward our results on the intentions to remain in Germany. To test our assumption, we first plot in Figure 3 the number of interviews around the events that we use in our main estimations. Figure 3 shows that there is no evidence of correlation between the implementation of the survey and the occurrence of events.

As a second test, we run a balance test on several respondents' characteristics that might be correlated with both the timing of the interview and the outcomes of interest. We regress each characteristic on the treatment status (i.e. interviewed after a terrorist event in the home country) and include year times country of origin fixed effects, as well as interview month fixed effects. The results are presented in Table 3. For all included characteristics there seems to be no difference between treatment and control groups. In Appendix Table C.1 we regress the treatment indicator on the full set of individual characteristics, and find that none of these characteristics significantly predicts the treatment status. In any case, we include the full set of individual characteristics in the main regressions.

Figure 3: Density of interviews around terror events



Notes: Figure 3 displays the distribution of events in the months around the events. All events and respondents from all waves are pooled together. The y axis indicates the total number of survey respondents for each blue dot, while the x axis indicates the months around terror events. The red line at 0 indicates the time of the terror event. Source: GSOEP

2.3 Main Results

In this section we present our main results for the effect of terrorism on intentions to remain in Germany. We first show graphical evidence on how intentions to remain evolve in the months around terror events. Figures 4 and 5 plot the event study coefficients, using as baseline the month before the event. We use two definitions for the outcome: intention to remain permanently (Figure 4) or intention to remain at least 5 years or more (Figure 5). Both plots show that coefficients for individuals interviewed before terror events are not statistically different from individuals interviewed in the month before the event, while coefficients are positive and statistically significant for immigrants interviewed after the event. Moreover, the plots show that the increase in intentions to remain lasts up to the fifth month after the attack.

Table 3: Balance in covariates

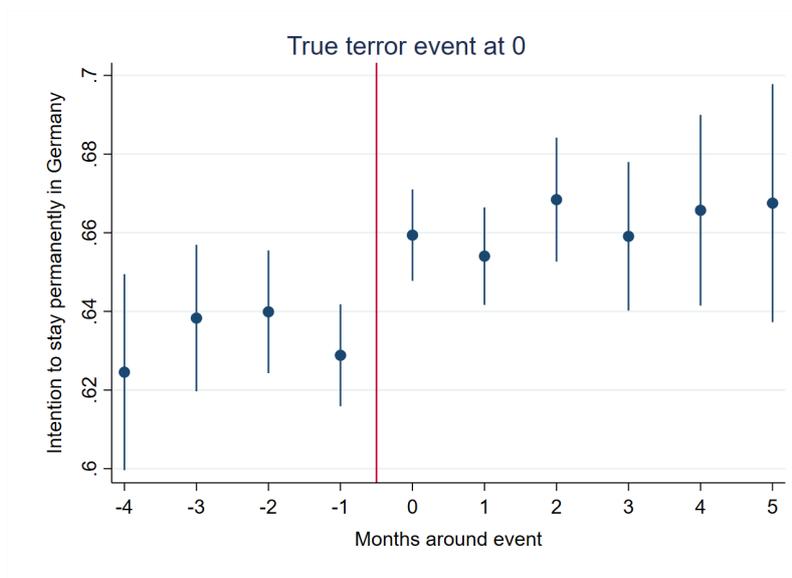
Covariate	N	Mean	Estimate	Std. Errors
Female	24743	0.49	-0.019	0.017
Age	24743	42.5	0.097	0.456
Years since mig.	24743	19.1	0.126	0.306
Low Sec. or below	24743	0.46	-0.009	0.016
Upper Secondary	24743	0.33	0.013	0.016
Post-Sec	24743	0.10	0.005	0.011
Higher education	24743	0.12	-0.010	0.010
Married	24743	0.73	-0.004	0.015
Full-time employ.	24743	0.45	0.011	0.017
Part-time employ.	24743	0.09	0.004	0.010
Other employ.	24743	0.06	0.003	0.008
Not employment	24743	0.40	-0.018	0.017
No children	24743	0.56	0.014	0.015
One child	24743	0.32	-0.011	0.015
Two or more child.	24743	0.12	-0.003	0.011

Notes: Table 3 displays the estimated coefficients for regressions of the terror treatment (interviewed before or after an event) on individual characteristics (4th column). Each coefficient comes from a separate regression. All regressions include terror event-specific fixed effects. The first column reports the name of the covariate, the second the size of the estimation sample, the third the mean value for the full sample, the fourth the estimated difference between treatment and control group, and the fifth robust standard errors for the coefficient.

$p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

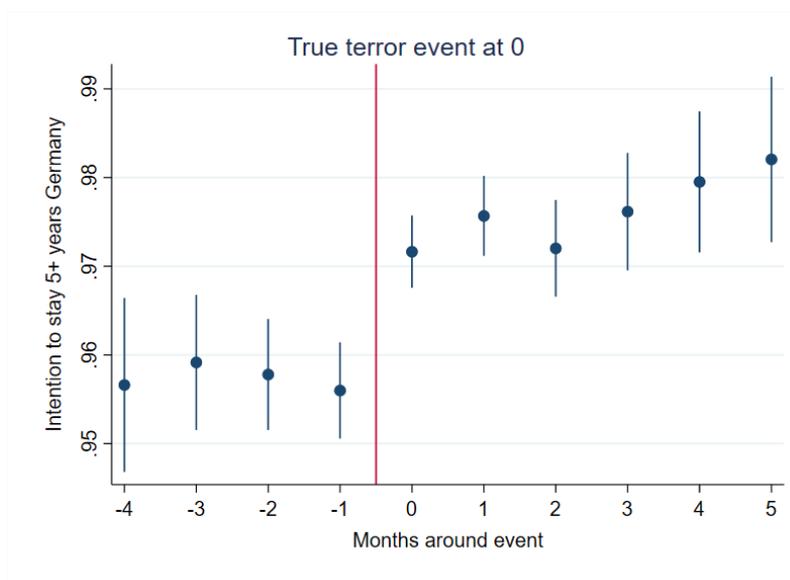
In Table 4, we report the results from regressions where the time indicator is substituted with a post-terror indicator that gives 0 to all periods before the event and 1 to all periods after the event. The bandwidth is 3 months around the event. Columns 1 to 4 display results for the intention to remain permanently, progressively including FE and individual controls. We estimate an effect of 3.7 percentage points. This corresponds to an increase of 5 percent relative to the mean value of the outcome variable (0.60). Column 5 reports the results with the full set of FE and individual controls for the intention to remain at least 5 or more years. We estimate an effect of 1.8 percentage points. Overall, the results suggest that the occurrence of terror events in the home country positively affects the intention to remain in the host country - Germany - longer or even permanently. In Section 3 we test whether changes in the intentions to remain in Germany affect the integration of immigrants in the labor market.

Figure 4: Effect of terror events on the intention to stay in Germany permanently



Notes: Figure 4 displays the event study plot from the estimation of Equation 6, where the outcome is "Remain in Germany more than 5 years". The regression includes country of origin FE, survey year FE, country of origin x survey year FE, month FE. Bars identify 95% confidence intervals.

Figure 5: Effect of terror events on the intention to stay in Germany 5+ years



Notes: Figure 5 displays the event study plot from the estimation of Equation 6, where the outcome is "Remain permanently in Germany". The regression includes country of origin FE, survey year FE, country of origin x survey year FE, month FE. Bars identify 95% confidence intervals.

2.4 Placebo Tests and Robustness Checks

In the previous section we showed that terror events in the home countries have a positive impact on the intentions to remain in Germany. In this section we test the stability of our results using both placebo tests and robustness checks. We apply two placebo tests. First, we consider country-of-origin-month-year combinations in which a terror event occurs. We then assign a placebo event to the country of origin-month-year combination of the previous year and the next year. For example, if there is a terror event in Turkey in April 2010, our placebo events are going to happen in Turkey in April 2009 and April 2011. After having defined placebo events, we assign survey respondents as either control or treatment groups for these events depending on whether they were interviewed before or after the occurrence of the placebo event. Following the example, we assign 0 to all Turkish respondents who were interviewed in the months before April 2009 and 2011, and 1 to all those interviewed after the same dates. Figure 6 shows the coefficients for the two placebo estimations, as well as our baseline coefficient on the true event.

Table 4: Terror events and intentions to remain in Germany

	Remain in GER (1)	Remain in GER (2)	Remain in GER (3)	Remain in GER (4)	Remain many yrs (5)
Terror	0.0300*** (0.00810)	0.0377*** (0.00830)	0.0376*** (0.00828)	0.0370*** (0.00827)	0.0176*** (0.00358)
Observations	24,743	24,743	24,743	24,743	24,728
R-squared	0.276	0.284	0.294	0.296	0.064
Origin country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes
Origin*Year FE	No	Yes	Yes	Yes	Yes
Controls 1	No	No	Yes	Yes	Yes
Controls 2	No	No	No	Yes	Yes

Notes: Table 4 displays the coefficients from the estimation of Equation 7. In Columns 1 to 4 the outcome is the probability of intending to remain in Germany permanently. In Column 5 is the probability of intending to remain at least 5 more years. Column 1 includes All regressions include country of origin FE, survey year FE, month FE. Column 2 includes also country of origin x survey year FE. Columns 4 and 5 include additional individual controls. These are gender, years since migration, marital status, employment status, education, age, federal state of residence. Robust standard errors are included in parenthesis. * $p < 0.1$; **

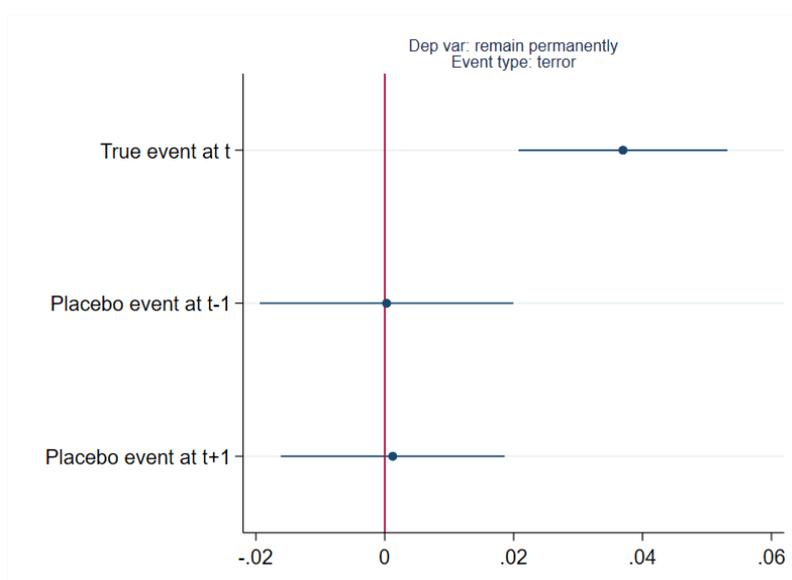
$p < 0.05$; *** $p < 0.01$

Source: GSOEP

We find no effects on the intention to stay in Germany permanently if the placebo event occurs one year before or one year after the true event. As a second placebo test we exploit the panel nature of the data and consider only immigrants for which we have observations in the year of an event and in the year before the event. In the survey year of the event we know whether these immigrants are treated or controls. We assign this treatment to the previous year, $y-1$, independently from whether they are treated or not in $y-1$. We then estimate the effect of being assigned a placebo treatment in $y-1$ on the intention to stay in $y-1$. Results are reported in Table 5. We show that there is no effect of the placebo treatment assignment on the intention to stay (Columns 1,3,5). Instead, we find a positive effect, very similar to the baseline results, when we run the regression for the true event, at $y = 0$, on the subsample of individuals that we observe in both t and $y-1$ (Columns 2,4 and 6).

Next, we test whether our results are driven by specific countries or survey years. We run the baseline regression excluding one survey year at a time, and repeat

Figure 6: Placebo tests: placebo event assignment



Notes: Figure 6 displays estimated coefficients and 95% confidence intervals (bars) for the baseline estimation in Table 4, Column 4, and the placebo estimations. The x axis reports the size of the estimated coefficient. Placebo estimations move the true event at t either to $t-1$ or $t+1$, in the same month and for the same country of origin as the true event. All regressions include the full set of fixed effects and individual controls. Robust standard errors.

Table 5: Placebo tests: placebo treatment assignment

	Treat = 0 in y-1		Treat = 1 in y-1		Any state in y-1	
	Placebo in y-1	True in y	Placebo in y-1	True in y	Placebo in y-1	True in y
Terror	0.0048 [0.013]	0.031 [0.023]	-0.015 [0.013]	0.037 [0.024]	-0.003 [0.009]	0.034** [0.015]
Observations	9502	9502	9886	9886	19423	19423
Individuals	5203	5203	5154	5154	7501	7501
R2	0.27	0.26	0.28	0.26	0.27	0.27

Notes: Table 5 reports the results from regressions where individuals who are treated at t are assigned the opposite treatment value at $y-1$. If they were treated in $y = 0$ the treatment indicator at $y-1$ will be 0. Only individuals who are present in the survey both at $y = 0$ and $y-1$ are included. All regressions include the full set of fixed effects and individual controls. Robust standard errors are included in parenthesis.

$p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Figure 7: Robustness: exclude one survey year at a time

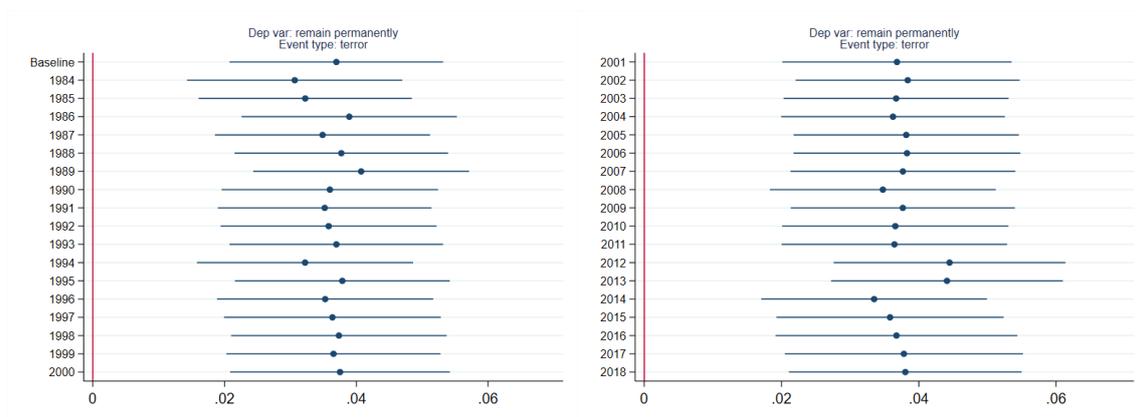


Figure 7 displays point estimates and 95% confidence intervals for regressions that exclude one survey year at a time. The y axis refers to the excluded survey year. The x axis indicates the size of the estimated coefficients. All regressions include the full set of fixed effects and individual controls as in the baseline estimation. Robust standard errors. Source: GSOEP

the same procedure excluding countries of origin. Figure 8 shows the estimated coefficients for each regression in which a survey year is excluded, while 8 shows the estimated coefficients for each regression in which a country of origin is excluded. The y axis displays the excluded survey year or country of origin. Overall, our results are stable throughout these robustness tests. Two notable exceptions are the exclusions of Turkey and survey years 2012-2013. When we exclude immigrants originally from Turkey, our effect drops to 2.0 percentage points. This drop may be attributed to the fact that Turkish migrants represent the largest portion of the sample and have a stronger attachment to Germany, so that the effect of terrorist events on intentions to stay are larger and weight more on the overall coefficient. Additionally, we test whether the main results are sensitive to the choice of the bandwidth around the event. In Figure 9, we display the estimated coefficients when we reduce the bandwidth from 3 months (i.e. the baseline bandwidth) to 1 month around the event, and when we extend it to 5 months around the terror events. The estimated coefficients remain similar to the baseline coefficient.

2.5 Heterogeneous Effects

In this sub-section we investigate if the effect of terror events on the intention to remain in Germany varies with the level of integration at the time of the event and

Figure 8: Robustness: exclude one country of origin at a time

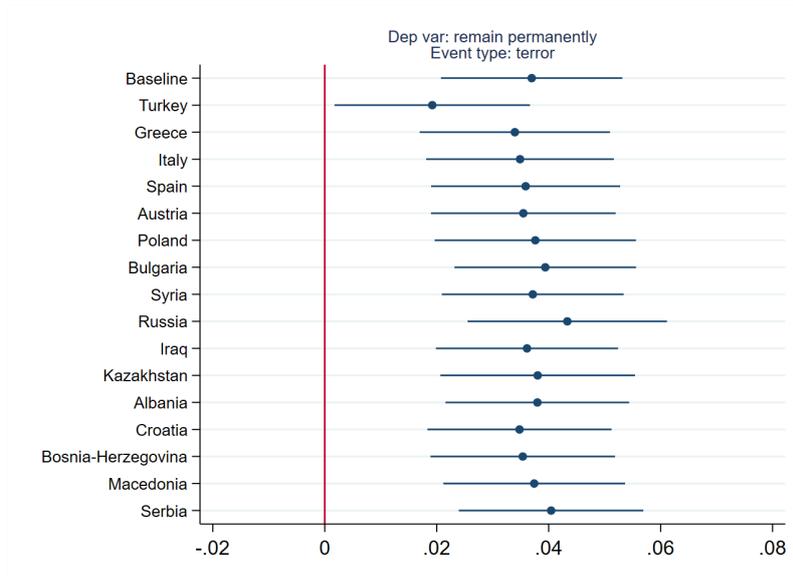


Figure 8 displays point estimates and 95% confidence intervals for regressions that exclude one country of origin at a time. The y axis refers to the excluded country of origin. The x axis indicates the size of the estimated coefficients. All regressions include the full set of fixed effects and individual controls as in the baseline estimation. Robust standard errors. Source: GSOEP

Figure 9: Robustness: alternative bandwidths around terror events

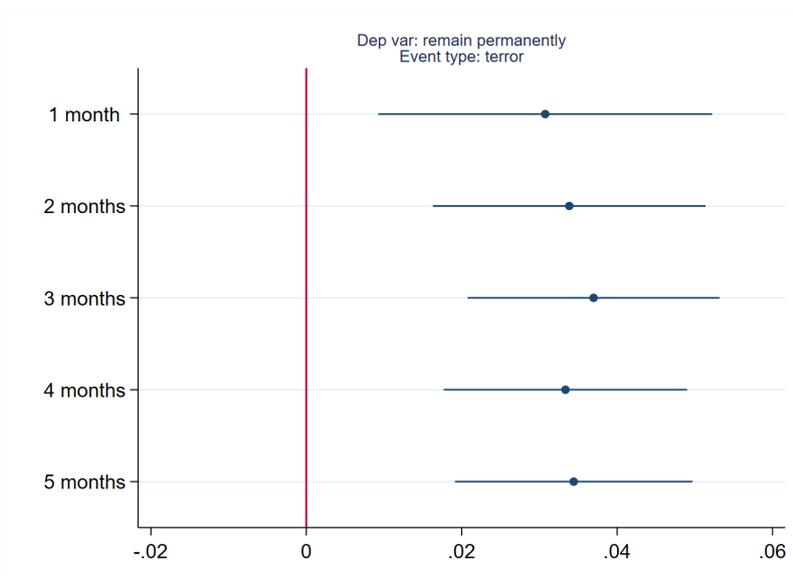


Figure 9 displays point estimates and 95% confidence intervals for regressions that lengthen or shorten the bandwidth of months around the event. The y axis refers to the size of the bandwidth around the event. The x axis indicates the size of the estimated coefficients. All regressions include the full set of fixed effects and individual controls as in the baseline estimation. Robust standard errors. Source: GSOEP

the diffusion of information from the country of origin to Germany. First, we test the hypothesis that the level of integration in Germany mediates the importance of terror events in the home countries in determining the willingness to remain in Germany. If immigrants are highly integrated in the German society is less likely that they pay attention to events occurring in their home countries.

We proxy the level of integration by the level of German knowledge. This is a self-declared measure that takes values "Very good", "Good", "Poor". Immigrants are asked to state the level of both their written and oral German. We take the average of these two measures. In case of midpoints (e.g. average of 2.5) we round up to the highest integer. We then run separate regressions for these three groups and display the coefficients in Figure 10. Our results show that for immigrants with a very good level of Germany (i.e. highly integrated in Germany) the effect of terror events on the intention to stay is virtually zero. On the contrary, for immigrants with good or poor German knowledge the effect is similar to our baseline results. Being interviewed after a terror event increases the intention to stay in Germany permanently by 3 percentage points.

As a second mediator, we test whether our results vary with the quality of information channels. We consider one specific information channel: internet. We assume that in periods and countries of origin with a broader internet diffusion, news about terror attacks spread more easily and faster not only within the country itself but also across the network of immigrants abroad.

We first collect data on internet penetration from the World Bank. This indicator is computed as the share of the population using internet in each year. From this continuous measure we create a binary variable that takes value of 1 internet penetration is higher than 50%. We then assign the binary measure for internet penetration to all observations based on the year of the interview and the country of origin, and run separate regressions for the two levels of internet penetration. To test also whether internet not only spreads more information, but also spreads it faster, we use the three-months bandwidth and the one-month bandwidth.

Figure 11 displays our estimated coefficients. For the one-month bandwidth we find that immigrants from countries with high internet penetration in the year of the event

react "faster" to terror events and increase their intentions to stay by 5.2 percentage points. If their countries of origin has low internet penetration the effect is small and not significant. When we increase the bandwidth to three months, we find that the effects are similar to our baseline results and that there is no difference in the estimated coefficients between low and high internet penetration. These results point to the importance of internet in spreading the information about terror events faster from the home country to the network of immigrants abroad. However, as months pass by, it is likely that other information channels (formal and informal) contribute to the spreading of information, so that initial differences by internet penetration disappear.

Finally, we test whether the persistence of terror events has an effect on the intensity with which immigrants react to these events. The effect of previous events is a priori ambiguous. On the one hand, if immigrants already experienced terror events, they might become accustomed to this type of violence and it would not affect their intentions to stay. On the other hand, immigrants may react more to a new terror event if this last event recalls the negative memories from previous events. To test this, we build an indicator that takes value 1 if in the year before the interview there was at least one terror event in the home country, and zero if there was no terror event. We then interact this indicator with the treatment. We display the results in Table 6. The interaction between terror and no event in previous year is around 0.05, while it is 0.02 if an event occurred in the previous year. Therefore, if no event occurred in the previous year the effect of terror on return intentions is larger.

3 Terrorism and Labor Market Integration

In the previous section we showed that the occurrence of terrorist events in the home country has a positive effect on the intentions to remain abroad. In this section we investigate whether the effects on intentions to stay translate into changes in the economic behaviour of migrants.

Previous research has shown that differences in the intended length of stay among immigrants can create different incentives to invest in human capital, which in turn lead to differences in earnings and career profiles (Adda et al., 2021). However, if we

Figure 10: Potential mechanisms: level of German knowledge

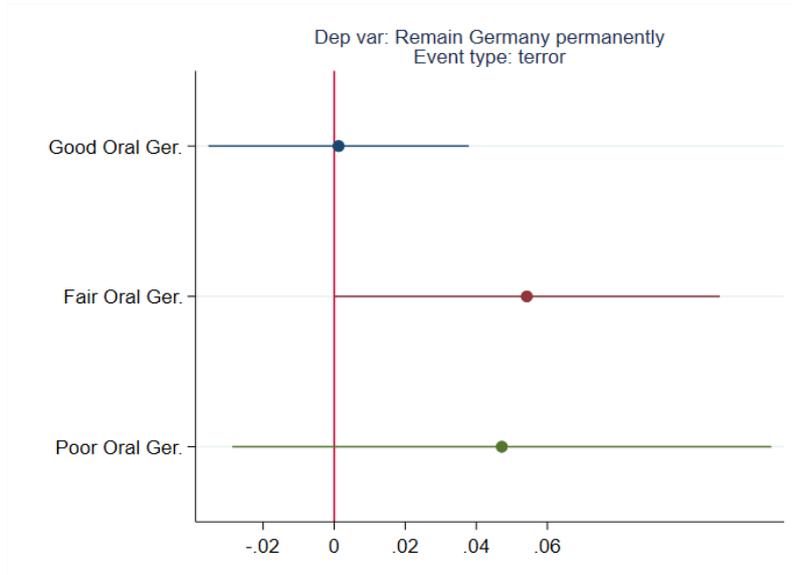


Figure 10 displays point estimates and 95% confidence intervals for separate regressions within the three levels of German knowledge. The y axis refers to the level of German. The x axis indicates the size of the estimated coefficients. All regressions include the full set of fixed effects and individual controls as in the baseline estimation. Robust standard errors. Source: GSOEP

Figure 11: Potential mechanisms: level of internet penetration

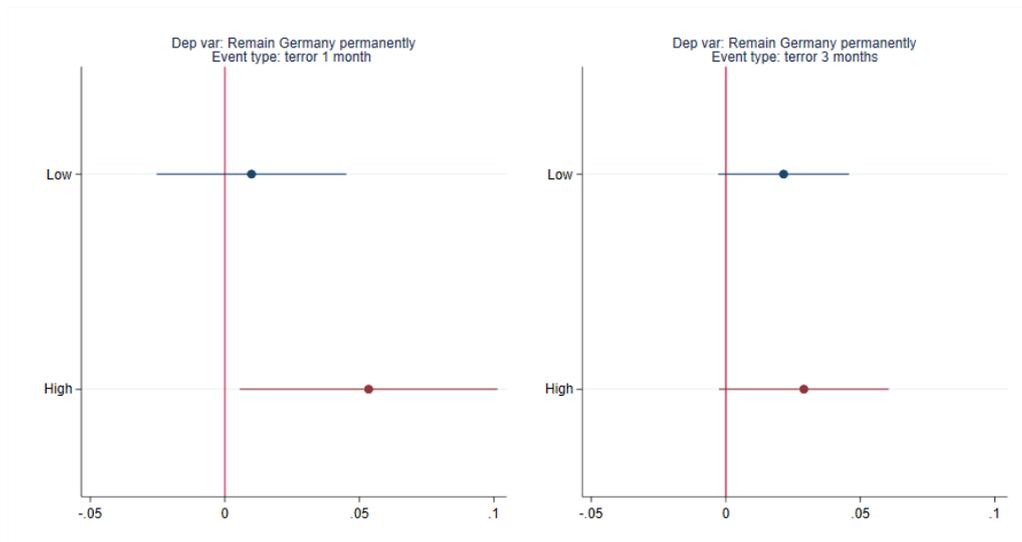


Figure 11 displays point estimates and 95% confidence intervals for separate regressions within the different levels of internet penetration in the home country. The y axis refers to the level of internet penetration in the home country. The left panel displays results using a bandwidth of 1 month around the event, while the right panel using a bandwidth of 3 months around the event. The x axis indicates the size of the estimated coefficients. All regressions include the full set of fixed effects and individual controls as in the baseline estimation. Robust standard errors. Source: GSOEP

Table 6: Potential mechanisms: occurrence of terror events in the previous year

	Remain in GER
	(1)
Terror*No country event pr. year	0.0583*** (0.0128)
Terror*Country event pr. year	0.0207** (0.00992)
Observations	19,104
R-squared	0.272
Event pr. year	Yes
Origin country FE*Year FE	Yes
Month FE	Yes
Controls	Yes

Notes: Table 6 reports the results of a regression in which the terror indicator is interacted with an indicator that gives value 1 to all respondents from a country of origin in which there was at least 1 terror event in the previous year and 0 to all respondents from a country of origin in which there was no event in the previous year. The regression includes the full set of fixed effects and individual controls. Robust standard errors are included in parenthesis. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

focus on these outcomes it is unlikely that we see an immediate change in response to a shock to return intentions. The completion of an educational degree⁸ or a change in the earnings path and career profile take time to materialize. Hence, it is empirically difficult to disentangle the true effect of terror events on these economic indicators. A measurable indicator of economic behaviour that reacts quickly to individual circumstances is job search activity among unemployed individuals. We take this as our preferred economic indicator. Unemployed individuals are also more likely to be immediately available to participate in re-training programs, which are offered on a more regular basis than a post-graduate or university degrees. Hence, the second indicator we look at is the participation in re-training measures (e.g., vocational training, further education) among unemployed individuals.

In this section we leverage social security data from Germany and test whether a negative shock to return intentions, induced by terror events, has an effect on the

⁸Investments in human capital observed in the GSOEP, such as enrolling in further education or acquiring an university degree, are measured once individuals have started to attend them rather than when the decision to take them was taken - and there can be a considerable lag between the two.

labor market outcomes of immigrants entering unemployment when terror events occur in their home countries. Specifically, we compare this group of immigrants to immigrants that enter unemployment in times of stable home country conditions and look at differences in time until employment (i.e. whether they put more effort in job search), wage at first job and participation on re-training programs.

The a priori effect of a negative event in the home country among unemployed migrants is less clear than the effect among employed migrants or recently arrived migrants who have some economic security. On the one hand, a negative shock has a positive effect on the intended length of stay which creates an incentive to invest in human capital to ensure a higher paying job in the now "longer" foreseeable future in the host country. In this case, we would expect to see unemployed migrants who experienced a terror event having higher participation in re-training measures than those who did not experienced a terror event. The effect on the unemployment duration is less clear, as migrants might prefer to wait to complete a training program before getting a job. On the other hand, a negative shock in the home country might create a "fear" among unemployed migrants of being obliged to return to the country of origin due to unsustainable economic conditions in the host country. In this case, home country terror events could create an incentive for unemployed migrants to find a job faster as a way to retain their visas or to economically ensure a stay in the host country, for instance⁹. If this is the case, we expect to observe that unemployed immigrants who experience a terror event have a shorter unemployment duration than those who did not experience a terror event. The effect on the participation on re-training measures is less clear as migrants might drop-off these programs to getting employment faster.

3.1 Data

Social Security Records The analysis relies on the social security records, *Integrated Employment Biographies* (IEB), for a random draw of 10% of the full population of immigrants in the German labor market. The Institute of Employment

⁹The amount of unemployment benefits an individual receives, and the duration, depends on how long you have been contributing and the salary you received before you became unemployed. Furthermore, individuals who have mini-jobs are not obliged to contribute to the unemployment insurance and self-employed individuals contribute on a voluntary basis.

Research (IAB) of the German Federal Employment Agency provides the data.¹⁰ The dataset includes detailed daily administrative longitudinal information on nationality, occupation, educational background, industry, employment status, and earnings records of all individuals subject to social security in Germany. Crucial for our empirical strategy, we have information on the precise date when immigrants enter unemployment, information on whether they take up training and the size of unemployment benefits. Given that the number of unemployed individuals in the GSOEP is relatively low and the questions regarding job search activity and the participation in unemployment programs are missing for a large share of the unemployed, IEB administrative data are better suited for this part of the analysis.

Terrorism For this part of the analysis, we rely on the absolute number of individuals affected by terror events at the time of the unemployment registration. The number of affected in terror events corresponds to the sum of killed and wounded. We aggregate daily data to the month-year-country level, and match terror data to the month-year date of unemployment registration in the IEB data. In the analysis, we use different levels of terror intensity based on the number of affected individuals. Specifically, we consider events that have at least 10,20,50,100 and 200 affected individuals. In Figure C.1 in Appendix, we plot the number of events within each category over time for all countries together. As a robustness check we also use relative measures of terrorist attacks - similar to those used in the analysis with the GSOEP - , as well as different definitions of terror (total number of events instead of total affected). Results are reported in Section 3.4.

3.2 Empirical Strategy

To investigate the effect of terror events on unemployed immigrants' labor market outcomes and human capital investments, we define immigrants from the same nationality who enter unemployment at the time of a terror event as treated and those who enter unemployment at the time of no events as controls. We therefore estimate the following model:

¹⁰For the description of a 2% random sample from the IEB, the *Sample of Integrated labor Market Biographies* (SIAB), see (Antoni et al., 2019).

$$Y_{i,t} = \text{Error}_t + \delta X_i + \gamma_o + \eta_{y,m} + \epsilon_{i,o,y,m} \quad (8)$$

where $Y_{i,t}$ is either employment probability, training take-up or unemployment benefits, Error_t is an indicator that takes value of 1 if a person from country of origin o entered unemployment in a month m and year y when terrorist events occurred in the country of origin o and 0 if a person entered unemployment in a month with no events, γ_o is country of origin fixed effects, η_y are month-year fixed effects, X is a set of covariates. As explained in the previous section we define Error_t using different levels of terror intensity, as well as alternative measures for robustness checks.

Our identifying assumption is that, had the terror event not occurred, the difference in outcomes between unemployed who entered unemployment with and without an event would have been zero. While we can't directly test this assumption, we run a balance test between these two groups of unemployed, comparing a large set of characteristics at the time of unemployment registration. Results are reported in Table 7, where the first column indicates the average values for the control group (i.e. those who entered unemployment in a month with no home country terror events¹¹), and the other columns indicate the difference between the control and the different treatment groups defined based on terror intensity. While some of the characteristics are statistically different, the size of the differences is extremely small and significance is given by the large sample size. For example, the age coefficient is always statistically significant. However, individuals in the treatment group are on average 0.0004 years younger than the control groups, a qualitative small difference.

3.3 Main Results

We now turn to our main results for the effect of terror on immigrants' labor market outcomes. We report results in Table 8. Columns I to V show the estimated coefficients from Equation 8, using unemployment duration (Panel A), retraining measures (Panel B) and unemployment benefits (Panel C) as outcomes. All

¹¹This follows our definition of no affected people in a terror event

Table 7: Balance in covariates among unemployed immigrants

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of affected individuals at unemployment registration					
	0	10	20	50	100	200
	Mean	Diff	Diff	Diff	Diff	Diff
Female	0.35	-0.00024 [0.007]	-0.00065 [0.0006]	-0.00026 [0.0006]	-0.00062 [0.0005]	-0.00077 [0.0004]
Vocational education	0.66	0.0016 [0.008]	0.0010 [0.007]	-0.00010 [0.007]	-0.00030 [0.006]	-0.0010 [0.0005]
Higher education	0.13	-0.0005 [0.001]	-0.00094 [0.001]	-0.0011 [0.001]	-0.0012 [0.0008]	-0.0019 [0.0007]
Age	36.18	-0.00041 [0.0004]	-0.00036 [0.0004]	-0.00036 [0.0004]	-0.00033 [0.0003]	-0.00027 [0.0002]
Experience	11.18	.00097 [0.0005]	.00090 [0.0005]	.00086 [0.0005]	.00069 [0.0004]	.00048 [0.0003]
Employed	0.01	-0.0045 [0.003]	-0.00427 [0.002]	-0.0020 [0.002]	.00042 [0.004]	-0.00019 [0.0017]
Earnings (zero included)	0.83	.00021 0.0006	0.00018 [0.0006]	.00013 [0.0006]	.00013 [0.0005]	0.00011 [0.0004]
UI (zero included)	13.12	-0.00009 [0.0002]	-0.00010 [0.0002]	-0.00010 [0.0002]	-0.000096 [0.0002]	-0.000087 [0.0002]
Re-training measures	0.04	-0.00086 [0.0015]	0.00022 [0.0015]	0.00022 [0.0015]	0.00001 [0.001]	0.00066 [0.0009]
Observations	443442	545844	520079	490439	475695	462569
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country of origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table 7 reports the estimated coefficients and the robust standard error in parenthesis from regression of the terror indicator on the full set of individual characteristics. Column 1 reports the mean values for the control group (immigrants who are registered in unemployment when no event in their home countries happens). In Columns 2 to 6 the definition of treatment changes based on the number of affected individuals from terror events. All regressions include country of origin FE, survey year FE, month FE.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

specifications include a set of individual characteristics, month-year fixed effects and country of origin fixed effects. For unemployment duration, we find that immigrants who enter unemployment in a month when at least 10 people were either killed or wounded in a terrorist attack have shorter unemployment duration. The decrease in unemployment duration is around 16 days with low-intensity terrorist events (at least 10 individuals affected) and goes down to 22 days with high-intensity terrorist events (at 200 individuals affected). For re-training measures, we find that terrorism reduces the probability of entering re-training measures during the unemployment spell. Finally, we find a negative effect also on unemployment benefits, which might be explained by the lower duration of the unemployment spell.

These results are consistent with the "fear" hypothesis among unemployed migrants. Terror events in the home country increase the desire to remain in Germany permanently and this creates an incentive among unemployed migrants to put more effort in search for a job. More effort reduces the unemployment duration. The negative effect on measures take-up is consistent with immigrants trading off these measures to enter employment faster.

3.4 Placebo Tests and Robustness checks

In this section we test the robustness of our results to a series of identification threats. One concern is that the effects on labor market outcomes are driven by other factors and would have occurred also in the absence of the terrorist event. To address this issue, we randomly assign the binary treatment status 500 times both across all observations and across observations who enter unemployment in the same year. That is, if across all observations there are x treated and y controls, the total number of treated and controls won't change, but x and y are reshuffled across observations. We then estimate the effect of placebo treatment status on unemployment duration. Figure 12 shows the distribution of the 500 estimated coefficients for the five definitions of treatment that we used in the main analysis (based on the terror intensity). The red vertical lines indicate the point under the true treatment assignment (these are the same coefficients reported in Panel A of Table 8). Figure 12 refers to the reshuffling within each year as it is a more restrictive criterium (instead of the reshuffling across all observations). For all treatment

Table 8: Effects of terror events on unemployed immigrants' outcomes

	(1)	(2)	(3)	(4)	(5)
	Number of affected individuals at unemployment registration				
	10	20	50	100	200
Panel A: Unemployment Duration					
Treated	-16.617 *** [1.81]	-16.619 *** [1.99]	-20.567 *** [2.38]	-21.153 *** [2.78]	-28.162 *** [3.47]
Panel B: Re-training measures during unemployment spell					
Treated	-0.0076 *** [0.002]	-0.0074 *** [0.003]	-0.0070 *** [0.003]	-0.0055 [0.0035]	-0.0016 [0.004]
Panel C: Unemployment benefits during unemployment spell					
Treated	-0.592 *** [0.08]	-0.737 *** [0.09]	-0.888 *** [0.10]	-1.026 *** [0.12]	-1.382 *** [0.15]
Observations	545844	520079	490439	475695	462569
Month-Year FE	Yes	Yes	Yes	Yes	Yes
Country of origin FE	Yes	Yes	Yes	Yes	Yes

Notes: Figure 9 reports the estimated coefficients and robust standard errors in parenthesis for regressions of the outcome on the terror indicator. The terror indicator is defined based on different levels of affected individuals in the home country in the same month when immigrants register as unemployed. Panel A uses unemployment duration as outcome, Panel B uses participation in re-training measures; Panel C uses unemployment benefits. All regressions include country of origin FE, survey year FE, country of origin x survey year FE, month FE. Individual controls: education, age, experience, gender. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: Integrated Employment Biographies (IEB)

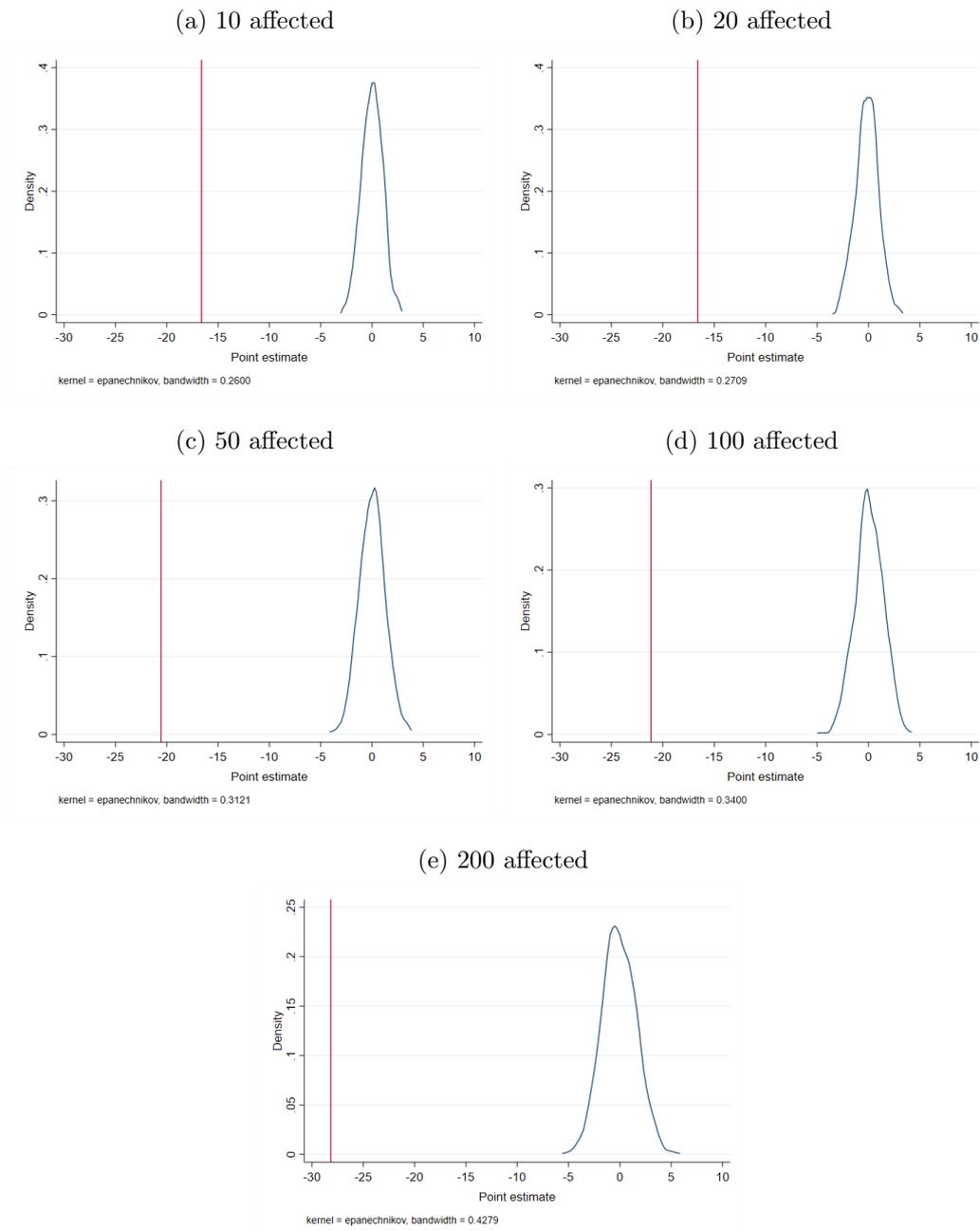
status definitions we find that assigning placebo treatment status to individuals who did not in reality experience a terrorist event has on average zero effects on their unemployment duration. This finding provide an important piece of evidence in favor of our baseline results. In the next sections we show that our estimated effects are robust to alternative samples and treatment definitions.

3.5 Displaced workers from mass-layoffs

In the previous section we show that immigrants who enter unemployment in months with large terror events are not statistically different in their baseline characteristics from immigrants entering unemployment in the absence of terrorist events. Nonetheless, one concern for our identification strategy remains if these two groups are different in terms of unobservables. If the timing of unemployment is endogenous to the time of events, our estimates on unemployment duration may be biased downward. In this section, we draw from the job displacement literature and restrict our analysis to immigrants who become unemployed due to mass lay-offs. We augment our administrative data with firm level information on mass lay-offs (Halla et al., 2020) and bankruptcies (Fackler et al., 2021). While the definition of mass lay-offs based on workers' flows allows us to relate to the larger literature on job displacement, the use of novel bankruptcy data allows for the inclusion of smaller firms and higher precision in the lay-offs timing. We assign a job displacement indicator to individuals who are registered as unemployed and had their last spell in a mass lay-off or bankruptcy establishment. We then run the same analysis as in the previous section on this subsample of displaced immigrants. The results from the balance test are displayed in Table C.2 in the Appendix. Overall, the sample is balanced on covariates. All coefficients are small in size and, if anything, with marginal significance. The average characteristics in the first column are also remarkably similar to those of the full sample of unemployed. Only female and highly educated immigrants are slightly less represented.

Our main results are reported in Table 9. We find that for displaced worker the effect of terrorism is stronger compared to the baseline. Precisely, we estimate a decrease of 32 days in unemployment at low-intensity terror, and of 38 days at high-intensity terror. The larger effects may suggest that our results for the full

Figure 12: Distribution of point estimates for placebo treatment assignments



Notes: Figure 12 displays the distribution of point estimates for 500 placebo treatment assignments. Placebo treatments are obtained by randomly re-assigning treatment status across all individuals in the same year. The red line indicates the true point estimate.
Source: Integrated Employment Biographies (IEB)

sample of unemployed are biased towards zero. We find also similar results for re-training measures and unemployment benefits although coefficients are smaller and mostly insignificant.

3.6 Alternative measures for terrorist violence

In this section we investigate the robustness of our results to alternative definitions of terrorist events. First, we address concerns related to the use of the absolute number of affected individuals as measure to assign our Terror treatment indicator. With the absolute number definition, if a country experiences a sequence of intense terrorist attacks individuals may be treated not just by events in the month when they enter unemployment but also in previous months. We therefore construct relative measures of terror that take into account mean and total terror events in the previous years. In particular, we assign our treatment indicator to individuals who become unemployed in a month where the number of affected individuals is three higher than either the previous year, two years or three years ¹². Using these criteria, we only consider treatment month-year-country of origin combinations that stand out in terms of affected individuals relative to recent past terror history of the country.

Second, we assign the treatment indicator based on the number of terror events in a month instead of the number of affected individuals. Again, we use relative measures. Results for unemployment duration are reported in Table 10. We find that using relative measures of terror, as well as alternative definitions of terror, leads to estimates of the effect on unemployment duration consistent with our baseline results. Experiencing a terrorist event in the home country reduces the time spent in unemployment.

4 Discussion and Conclusion

In contrast to past migration patterns, contemporary migration tends to be a temporary phenomena. Migrants move more frequently across countries and between host and home countries. The economic and social behavior of temporary migrants

¹²Note that we vary also by how much should the number of affected individuals be higher than average or total of the previous months/years.

Table 9: Effects of terror events on displaced immigrants' outcomes

	(1)	(2)	(3)	(4)	(5)
	Number of affected individuals at unemployment registration				
	10	20	50	100	200
Panel A: Unemployment Duration					
Treated	-32.559 *** [7.90]	-29.497 *** [8.77]	-35.878 *** [10.44]	-37.058 *** [12.18]	-38.498 ***
Panel B: Re-training measures during unemployment spell					
Treated	-.0148 [0.008]	-.0134 [0.009]	-.0070 [0.011]	-.00047 [0.013]	.0183 [0016]
Panel C: Unemployment benefits during unemployment spell					
Treated	-.709 *** [0.27]	-.744 * [0.30]	-.540 [0.36]	-.324 [0.42]	-.545 [0.52]
Observations	41553	39463	37098	35952	34990
Month-Year FE	Yes	Yes	Yes	Yes	Yes
Country of origin FE	Yes	Yes	Yes	Yes	Yes

Notes: Figure 9 reports the estimated coefficients and robust standard errors in parenthesis for regressions of the outcome on the terror indicator. The terror indicator is defined based on different levels of affected individuals in the home country in the same month when immigrants register as unemployed. Panel A uses unemployment duration as outcome, Panel B uses participation in re-training measures; Panel C uses unemployment benefits. All regressions include country of origin FE, survey year FE, country of origin x survey year FE, month FE. Individual controls: education, age, experience, gender.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 10: Effects of terror events on displaced immigrants' outcomes

	(1)	(2)	(3)
	Higher than the average of last:		
	12 months	24 months	36 months
Panel A: Relative measures for number of affected individuals			
Treated	-8.98 *** [2.65]	-11.95 *** [2.62]	-13.57 *** [2.57]
Observations	391950	392488	394518
Panel B: Relative measures for number of events			
Treated	-7.41 *** [2.50]	-5.91 *** [2.39]	-7.95 *** [2.31]
Observations	391157	394957	398175
Month-Year FE	Yes	Yes	Yes
Country of Origin FE	Yes	Yes	Yes

Notes: Table 10 reports the estimated coefficients and robust standard errors in parenthesis for regressions of the outcome on the terror indicator. The terror indicator is defined based on relative measures of terror. In Panel A we use the number of affected individuals, in Panel B the total of terrorist events. We assign treatment status to month-year-country of origin combinations for which the number of affected individuals or total terror events is higher than the average of the past 12 months (column 1), 24 months (column 2) and 36 months (column 3). All regressions include country of origin FE, survey year FE, country of origin x survey year FE, month FE. Individual controls: education, age, experience, gender.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

can be sharply different from that of permanent or long-term migrants. Previous research has shown that differences in the intended length of stay among immigrants can create different incentives to invest in human capital, which in turn lead to differences in earnings and career profiles (Adda et al., 2021). It is therefore important to develop a better understanding of the determinants of migrants intended length of stay. In this paper we contribute to this understanding. Specifically, we investigate whether home country socio-political conditions affect immigrants return intentions and labor market behavior in the host country. We focus on terrorist events in the home country and combine precise terror event data with survey and administrative data. Our paper is the first to test empirically the effect of changes in home country conditions on return intentions and labor market outcomes. We show that terrorist attacks in the home country have an immediate effect on return intentions, increasing the willingness to remain in the host country, and on job search behaviors, reducing the time in unemployment. Our results add an important and credible piece of evidence on the effect of home-country events of migrants' behaviour. With this study, we contribute to the understanding of migrants intended duration of stay and effort to integrate in the host-country society. Our insights are policy-relevant for both host and home countries since they help host countries to understand what affects migrants integration and home countries how they might attract migrants back home.

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Conclusion

In many countries, migration is a high priority in both the public and political debate. Countries face continuous challenges to attract, retain and integrate migrants in their societies. In recent years, the economic crisis, the refugee crisis and the surge of populist movements and xenophobic violence have exposed countries to even bigger challenges. It is therefore crucial to understand what governments and societies can do to transform migration phenomena into opportunities for both destination and origin countries.

Following this idea, in my dissertation I investigated empirically three topics in the economics of migration. First, the role of certificate recognition for the labor market integration of high-skilled migrants and the effects of a policy that facilitated certificate recognition for all immigrants. Second, the brain drain phenomenon and the effect of a policy that introduce tax incentives to return migration for high-skilled young Italian emigrants. Third, return intentions and labor market behaviors of immigrants, and the effects of home country socio-political conditions on these two outcomes.

In Chapter 2, the first empirical chapter, I evaluated the effect of the Federal Recognition Act on the integration of non-EU immigrants. The reform, passed in Germany in 2012, introduced a legal basis for all immigrants with vocational and professional qualifications acquired abroad to recognize their certificates in Germany and enter regulated occupations (e.g. health care occupations). Exploiting the fact that only non-EU immigrants experienced a change in legislation, I use a Differences-in-differences design and show that a) the reform was successful in increasing recognition rates for non-EU, b) the reform increased the probability of being employed in regulated occupations, improving the economic situation of non-EU immigrants, c) the larger inflow of non-EU immigrants in regulated occupations did not only occur in occupations suffering from skill-shortage. Nonetheless the quality of non-EU entering these occupations did not decline, neither in terms of labor market characteristics, nor in terms of earnings in regulated occupations.

With this chapter, I contribute to different strands of research. First, I show that a large-scale policy with relatively low implementation costs can be effective in improving the economic integration of a large number of immigrants. Second, I show that returns to recognition hold across different occupations and there seems to be no trade-off between better integration and quality of immigrants entering regulated occupations. My results are also policy relevant as certificate recognition is on top of the policy agenda in many destination countries, to unleash the unexploited potential of the migrant workforce.

In my second empirical chapter (Chapter 3), I evaluate the effect of the Controesodo law, passed by the Italian government in 2010. The law introduced large (70% - 80%) tax deductions for Italians coming back to Italy from abroad, as long as they had a higher education certificate, had spent at least two years abroad and were born after 1969. Exploiting these discontinuities, I provide evidence on the effect of the law using both Italian census data and German social security records (Germany is the most frequent destination country for Italians together with the UK). I show that the law increased the return rates of eligible Italians, and find similar results when I use the probability of leaving Germany as the focus of the analysis. Additionally, I investigate whether some groups benefited more from the law. In contrast to previous research, I show that the law was effective in attracting emigrants from the whole earnings distribution (not only the richest) and from specific sectors only (e.g. health care, engineering). The findings in this chapter contribute to the literature on taxation and brain drain by evaluating a unique setting where the government tried to reduce excessive emigration via fiscal incentives. Moreover, they provide important insights for policy makers on the heterogeneous effects of tax incentives to return migration.

Finally, in Chapter 4, I take a broader perspective on the determinants of return intentions and integration in the host country. Most of the previous work has focused on individual determinants of return migration or on contextual factors in the destination country (xenophobic violence, economic booms and downturns). The previous chapter provided causal evidence that policies in the home country may affect

immigrants' return plans. In this chapter, I show that also home country conditions in terms of socio-political (in-)stability play a role in migrants' decision to return and integrate economically. Taking terrorist attacks as shocks to the socio-political conditions at home, and combining them with detailed survey and administrative data, my results indicate that a) terrorist attacks have a direct effect on return intentions, increasing the willingness to remain abroad. b) terrorist attacks have a direct effect on the job search effort of unemployment immigrants, as proxied by the time to re-employment.

These findings provide a first empirical evidence on the role of socio-political stability (and not just economic factors) on the decision to migrate, giving valuable insights - also for policy makers - on the integration decisions of immigrants.

Taken together, my dissertation provides rigorous empirical evidences on the effects of policies and macro-level determinants of migration and migrants' integration in the host labor market. The dissertation contributes to the growing literature in the economics of migration with novel findings on specific policies and channels, focusing not only on their direct effects, but also on the heterogeneity of their effects, and possibly their unintended consequences. The findings in my dissertation may also be highly policy relevant for the future analysis of migration and integration policies, but also for the development and improvement of new policies.

Appendix

A Appendix: Chapter 2

A.1 Additional Figures and Tables

A.1.1 Figures

Figure A.1: Example of the available information on the website "Recognition in Germany"

Your recognition procedure as General nurse (m/f) in Berlin, Berlin

What I know already

- The profession of General nurse (m/f) is **regulated** in Germany.
- Recognition is necessary in order to be able to work in the profession in Germany.
- Since 1 January 2020 the German profession is called "Pflegefachfrau" or "Pflegefachmann".

Quick-Info

- Name of the procedure
- Requirements for recognition
- Knowledge of German
- Duration
- Costs

Documents for my application

The competent authority

Landesamt für Gesundheit und Soziales Berlin

Turmstraße 21
10559 Berlin

[View on Google Maps](#)

+49 30 90229 0

E-Mail

[Website](#)

Your contact

E-Mail

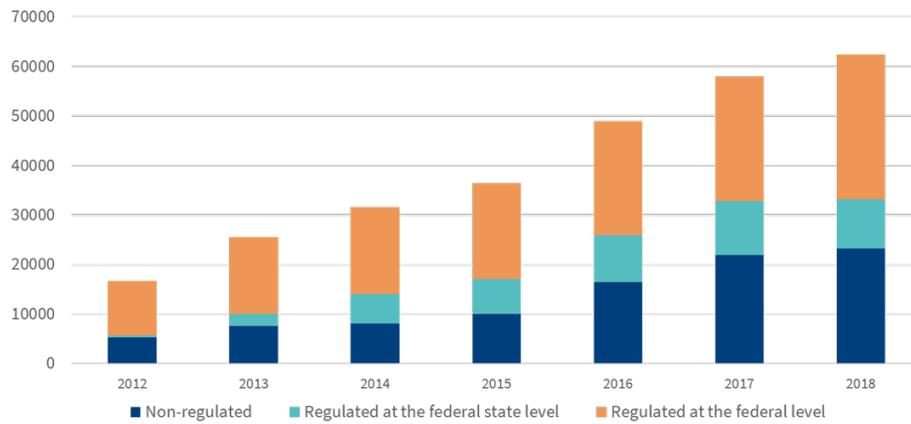
Telefonsprechzeiten:

Dienstag und Donnerstag von 13:00 Uhr bis 15:00 Uhr

Besuchszeiten:
Beratung und Abgabe von Unterlagen ist nur nach Terminvereinbarung möglich
Terminanfragen bitte per E-Mail

Notes: Figure A.1 is a screenshot of the webpage www.anererkennung-in-deutschland.de that results from the search of nursing jobs in Berlin. The webpage provides information on the type of certificate required and on the recognition procedure to follow. Source: website www.anererkennung-in-deutschland.de

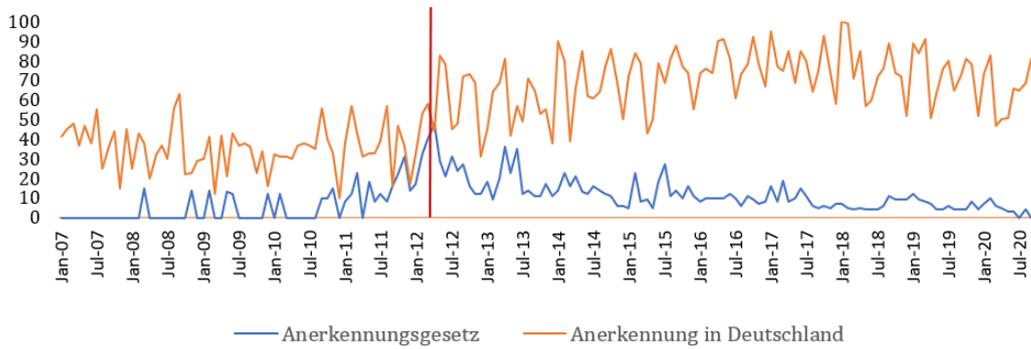
Figure A.2: Total number of applications by type of occupation for which recognition is requested



Notes: The figure shows the total number of applications by year and type of occupation for which recognition is requested. Occupations can be regulated at the federal (*Bundesebene*) or state level (*Landesebene*) or non-regulated. Non-regulated jobs for which application is possible include all vocational occupations (*Ausbildungsberufe*). Data on recognition outcomes or applications is not available before 2012.

Source: BIBB, Official statistics on the Federal Recognition Act.

Figure A.3: Google searches about recognition of foreign certificates in Germany



Notes: Figure A.3 displays the amount of google searches for recognition in Germany (*Anerkennung in Deutschland*) and Federal Recognition Act (*Anerkennungsgesetz*) between 2007 and 2020. Data are restricted to searches made in Germany. Searches are normalized to 100 in the peak period. The red vertical line indicates the day in which the Federal Recognition Act came into force (April 1st 2012).

Source: Google Trends (searched on 13.11.2020).

Figure A.4: Share of applicants by year and nationality group

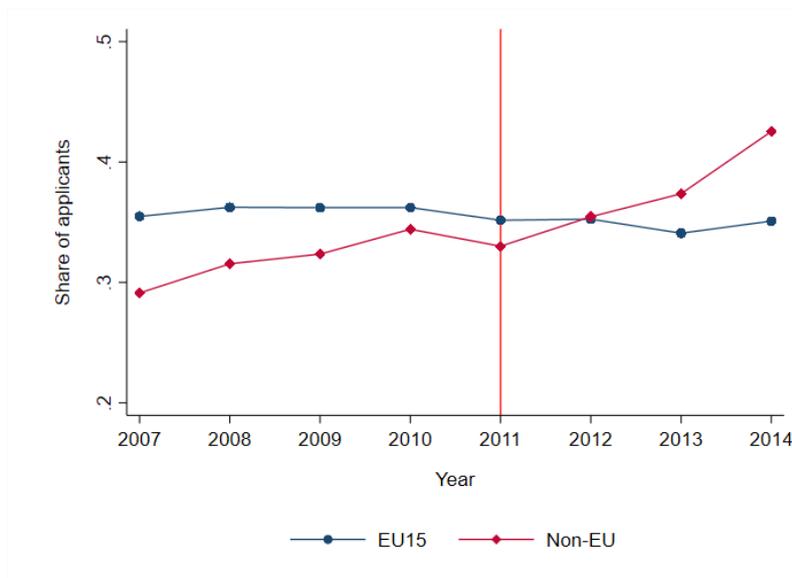
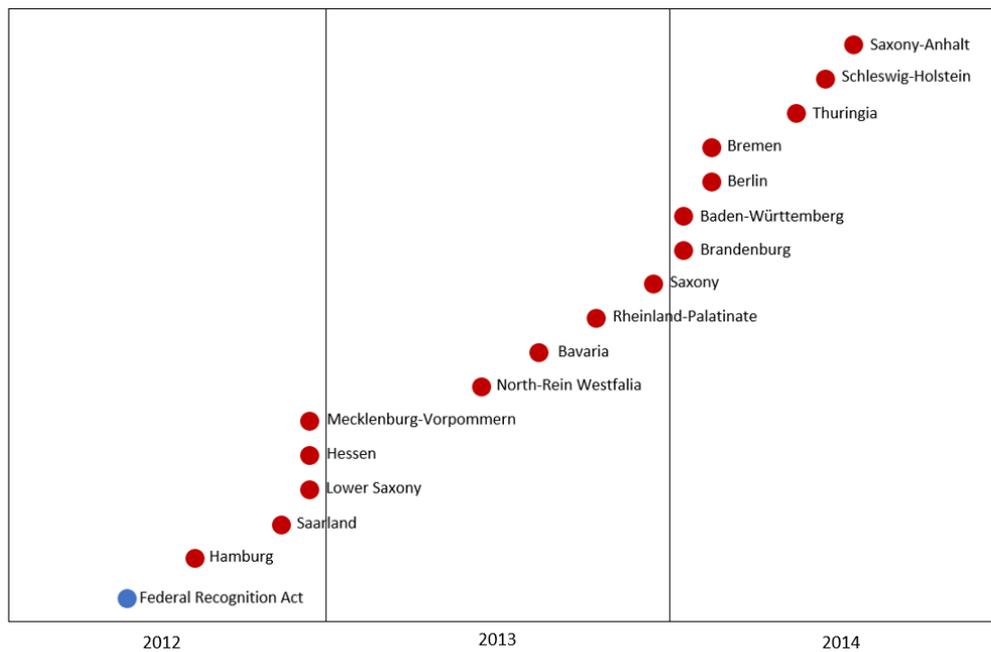


Figure A.4 displays the share of EU (blue) and non-EU (red) immigrants who applied for recognition in each year from 2007 through 2014. The vertical line indicates the year before the recognition reform (2011). Shares are computed combining information on the year of application and the year of arrival in Germany. The denominator includes all eligible EU15 and non-EU immigrants who stated they were in Germany in year t .

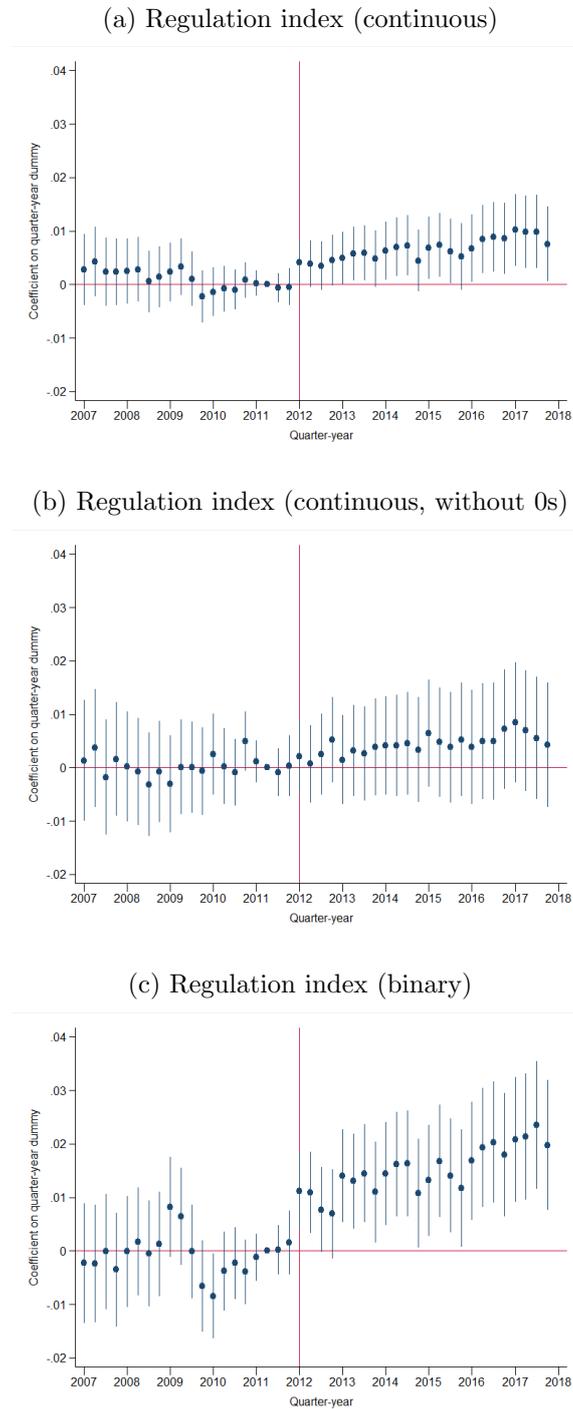
Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016

Figure A.5: Timing of the introduction of recognition laws across federal states.



Notes: Figure A.5 displays the timing of state recognition laws from 2012 to 2014. The blue dot is the Federal Recognition Act (nation-wide recognition law). Source: Own graphical representation from BIBB data (2015)

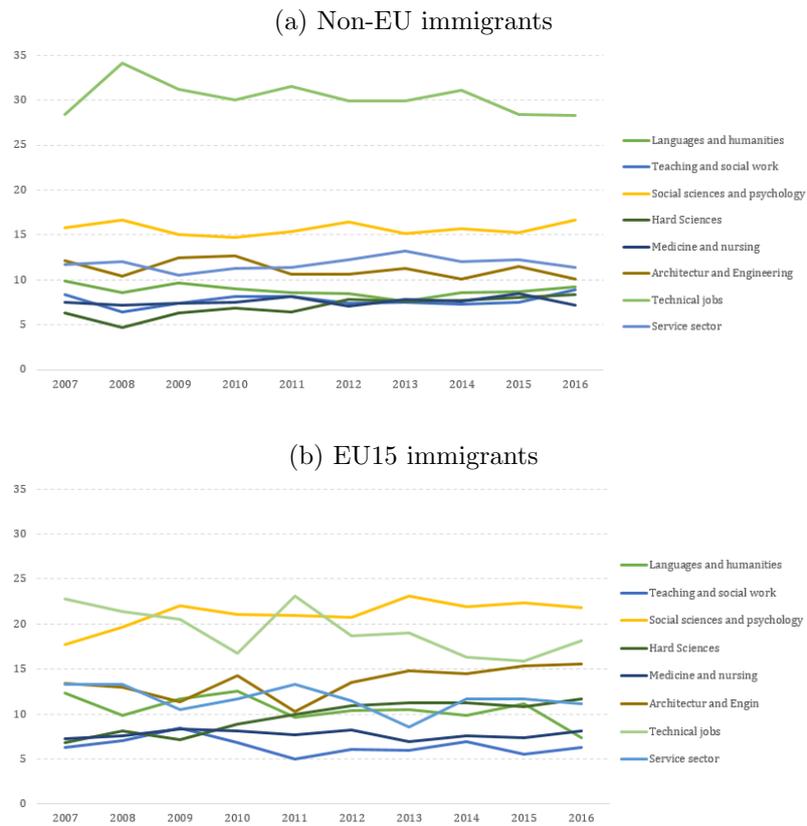
Figure A.6: Event study plots for alternative definitions of the regulation index as outcome



Notes: Figure A.6 shows the estimated coefficients and the 95% confidence intervals for regression models where quarters of year are interacted with the nationality dummy. The outcomes are the continuous regulation index measure (Panel a), the continuous regulation index without zeros (Panel b), a dummy variable that takes value 1 if the regulation index is higher than 0, and 0 otherwise (Panel c). Coefficients are estimated for each quarter pre- and post reform. The baseline is March 2011. Each coefficient represents the difference between EU15 and non-EU immigrants in percentage points from the baseline difference in outcomes.

Source: Integrated Employment Biographies.

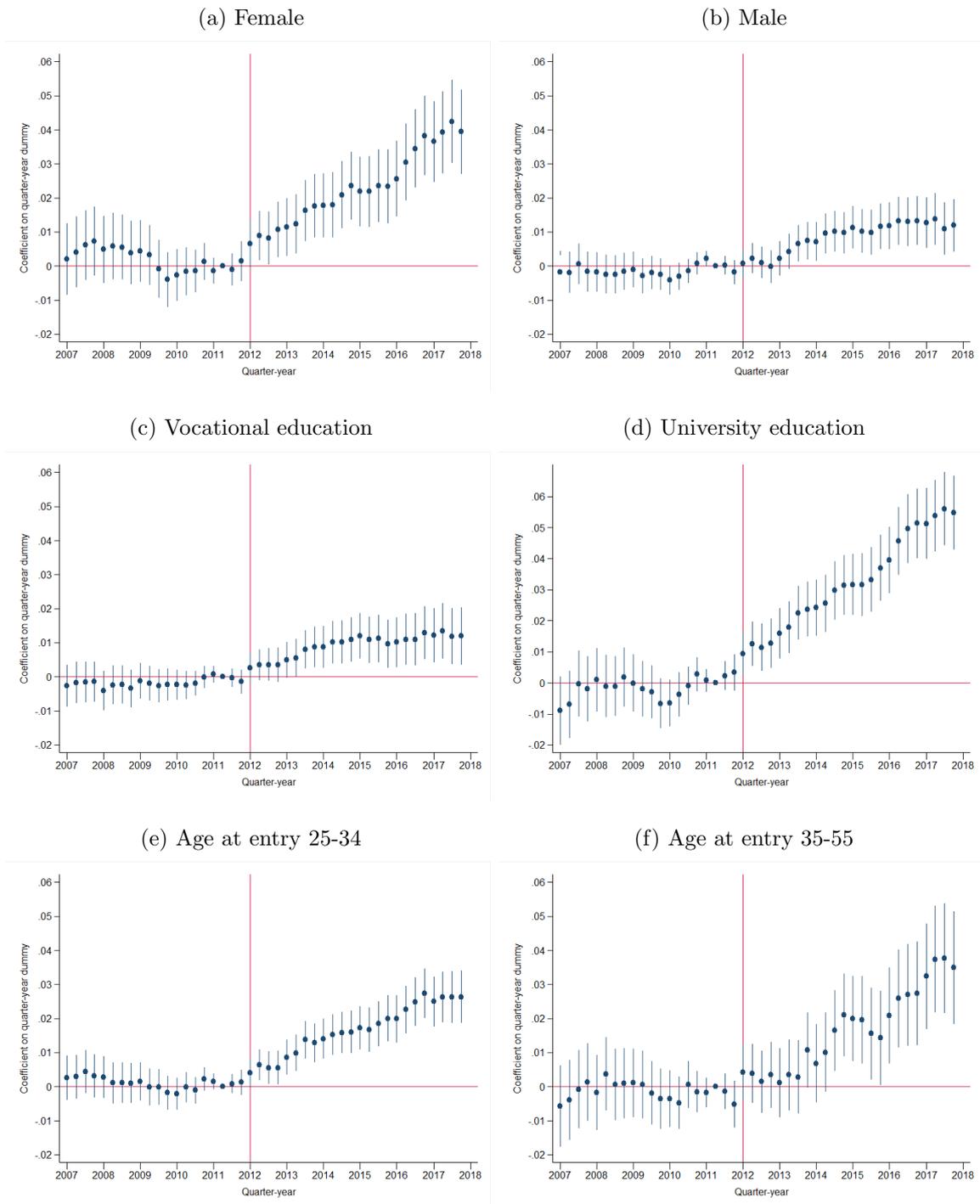
Figure A.7: Evolution of field of study for non-EU and EU15 immigrants



Notes: Figure A.7 displays the distribution of fields of study between 2007 and 2016 for non-EU (Panel a) and EU15 (Panel b). Data come from the German Microcensus and the sample is the same as in Table 2. Technical jobs are jobs in architecture and engineer for immigrants with highest education VET (these are technician certifications for example).

Source: German Microcensus, 2007-2016.

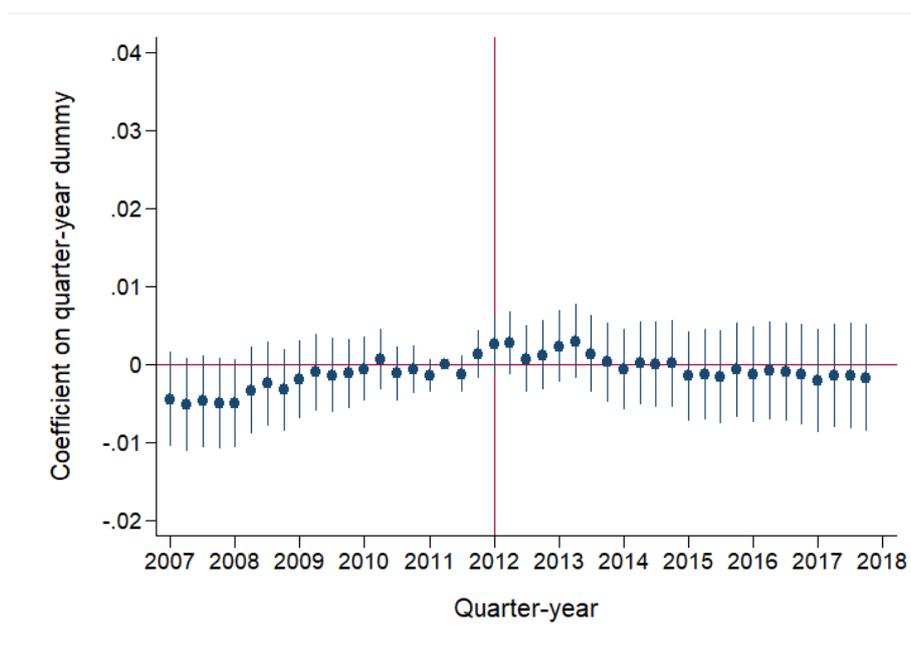
Figure A.8: Heterogeneous effects across individual characteristics



Notes: Figure A.8 shows the estimated coefficients and the 95% confidence intervals for regression models where quarters of year are interacted with the nationality dummy, and only subgroups of immigrants are included. The subgroup is stated on top of each plot. In all plots the outcome variable is the probability of being employed in regulated occupations with many applications. are the continuous regulation index measure (Panel a), the continuous regulation index without zeros (Panel b), a dummy variable that takes value 1 if the regulation index is higher than 0, and 0 otherwise (Panel c). Coefficients are estimated for each quarter pre- and post reform. The baseline is March 2011. Each coefficient represents the difference between EU15 and non-EU immigrants in percentage points from the baseline difference in outcomes.

Source: Integrated Employment Biographies.

Figure A.9: Effect of Recognition Act on employment in non-regulated *Mangelberufe*



Notes: Figure A.8 shows the estimated coefficients and the 95% confidence intervals for regression models where quarters of year are interacted with the nationality dummy. The outcome variable is the probability of being employed in non-regulated occupations with skill shortage and ineligible for recognition (*Mangelberufe*). Coefficients are estimated for each quarter pre- and post reform. The baseline is March 2011. Each coefficient represents the difference between EU15 and non-EU immigrants in percentage points from the baseline difference in outcomes.

Source: Integrated Employment Biographies.

A.1.2 Tables

Table A.1: Why do immigrants not apply for recognition?

	(1)	(2)	(3)	(4)
	All immigrants		Arrived pre-reform	
	EU15	Non-EU	EU15	Non-EU
	in %		in %	
Administrative constraints	13.68	23.94	14.57	23.48
No perspective of recognition	14.74	19.69	14.57	20.00
Not important	38.42	32.43	35.76	33.48
Other reasons	33.16	23.94	35.1	23.04
Observations	190	259	151	230

Notes: Table A.1 reports the percentage of immigrants who would have been eligible for recognition but did not apply according to the reasons for no application aggregated in four groups: administrative constraints, no perspective of recognition, not important or other reasons. Responses come from a question ask in all waves of the IAB-SOEP Migration Survey on the reasons why immigrants did not apply for recognition of their vocational or university certificate acquired abroad. In the first two columns all EU15 and non-EU immigrants for which the information is available are included. In the last two columns only EU15 and non-EU immigrants who entered Germany before the reform are included.

Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.2: Effects of the Federal Recognition Act on the probability of applying for recognition

	(1)	(2)	(3)	(4)	(5)	(6)
	All immigrants			Arrived pre-reform		
Panel A: Applications						
Post* Non-EU	0.067** [0.03]	0.055** [0.02]	0.53** [0.02]	0.048* [0.03]	0.043* [0.02]	0.41* [0.02]
Baseline (Non-EU)	0.32	0.32	0.32	0.32	0.32	0.32
R2	0.002	0.22	0.24	0.004	0.23	0.24
Individuals	797	797	797	682	682	682
Observations	4822	4822	4822	4533	4533	4533
Panel B: Closed recognition procedures						
Post*Non-EU	0.067** [0.03]	0.052** [0.02]	0.048** [0.02]	0.044* [0.03]	0.037* [0.02]	0.036 [0.02]
Baseline (Non-EU)	0.28	0.28	0.28	0.28	0.28	0.28
R-squared	0.003	0.21	0.23	0.006	0.21	0.23
Individuals	797	797	797	682	682	682
Observations	4822	4822	4822	4533	4533	4533
Individual controls	No	Yes	Yes	No	Yes	Yes
Year FE	No	No	Yes	No	No	Yes
State FE	No	No	Yes	No	No	Yes

Notes: Table A.2 reports coefficients from our main regression model using as outcome the probability of applying for recognition (Panel A) and the probability of completing a recognition procedure (Panel B). Columns 1,2 and 3 report results for the full sample of immigrants, columns 4,5 and 6 for the subset of immigrants who arrived in Germany in the pre-reform period. Controls include sex, age, time since migration, nationality. Year and state (*Land*) fixed effects are included. Robust standard errors. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$ Source: IAB-SOEP Migration Sample, waves 2013,2014,2015,2016.

Table A.3: Event study for the effect of the reform on regulated occupations

	(1)	(2)	(3)	(4)	(5)	(6)
	All eligible regulated occupations		Regulated occupations (high number of applicants)		Regulated occupations (low number of applicants)	
t = -5	-0.003 [0.003]	-0.004 [0.003]	0.000 [0.003]	0.002 [0.003]	-0.004 [0.002]	-0.005 [0.002]
t = -4	-0.001 [0.003]	-0.000 [0.003]	0.000 [0.002]	0.002 [0.003]	-0.001 [0.002]	-0.001 [0.002]
t = -3	-0.002 [0.003]	-0.001 [0.002]	0.001 [0.002]	0.000 [0.002]	0.001 [0.002]	0.001 [0.002]
t = -2	0.003 [0.002]	-0.002 [0.002]	-0.002 [0.002]	-0.001 [0.002]	-0.001 [0.001]	-0.001 [0.001]
t = 0	0.005 *** [0.002]	0.005 *** [0.002]	0.005 *** [0.002]	0.005 *** [0.002]	0.000 [0.001]	0.001 [0.002]
t = +1	0.011 *** [0.003]	0.011 *** [0.002]	0.010 *** [0.002]	0.010 *** [0.002]	0.001 [0.002]	0.000 [0.002]
t = +2	0.015 *** [0.003]	0.015 *** [0.003]	0.015 *** [0.002]	0.015 *** [0.002]	-0.001 [0.002]	0.001 [0.002]
t = +3	0.018 *** [0.003]	0.017 *** [0.003]	0.018 *** [0.003]	0.017 *** [0.003]	-0.000 [0.002]	0.001 [0.002]
t = +4	0.024 *** [0.004]	0.023 *** [0.004]	0.023 *** [0.003]	0.022 *** [0.003]	-0.000 [0.002]	0.001 [0.002]
t = +5	0.028 *** [0.004]	0.026 *** [0.004]	0.027 *** [0.003]	0.025 *** [0.003]	-0.001 [0.002]	0.002 [0.002]
Baseline (Non-EU)	9.27	9.27	6.76	6.76	2.51	2.51
R-squared	0,006	0,056	0,002	0,052	0,002	0,028
Individuals	76499	76499	76499	76499	76499	76499
Observations	1898060	1898060	1898060	1898060	1898060	1898060
Individual Controls	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
LLM FE	No	Yes	No	Yes	No	Yes

Notes: Table A.3 reports event study coefficients from the interaction between the non-EU dummy and years. The outcome is the probability of being employed in all regulated occupations (columns 1 and 2), in regulated occupations with many applications (columns 3 and 4) and in regulated occupations with few applications (columns 5 and 6). The baseline year is 2011 (one year before the reform). Individual controls and group fixed effects are the same as in the main specification. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies

Table A.4: Main specification with only employed and only full-time employed

	(1)	(2)	(3)	(4)	(5)	(6)
	Only employed			Only full-time employed		
Post*Non-EU	0.013*** [0.003]	0.012*** [0.003]	0.002 [0.002]	0.011*** [0.002]	0.012*** [0.002]	-0.002 [0.002]
Baseline (Non-EU)	12.7	9.3	3.4	12.6	9.2	3.4
R-squared	0.069	0.066	0.031	0.083	0.087	0.026
Individuals	70079	70079	70079	54737	54737	54737
Observations	1554852	1554852	1554852	1038171	1038171	1038171
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.4 reports the coefficients from our main regression model excluding unemployed immigrants. The outcome variable is the probability of working in regulated occupations. Column 1 and 4 are eligible regulated occupations, 2 and 5 are regulated occupations with many applicants, 3 and 6 are regulated occupations with few applications. Columns 1,2,3 refer to all employees (full time and part time), columns 4,5,6 only to full-time employees. Individual controls and group fixed effects are the same as in the main specification. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies

Table A.5: Main specification with balanced panels

	(1)	(2)	(3)	(4)
	2007-2017	2008-2017	2009-2017	2010-2017
Post*Non-EU	0.019*** [0.003]	0.017*** [0.003]	0.015*** [0.003]	0.015*** [0.002]
Baseline (Non-EU)	7.30	7.41	7.45	7.73
R-squared	0.06	0.06	0.06	0.06
Individuals	20010	21023	22831	24490
Observations	754053	724445	708849	683656
Individual controls	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes

Notes: Table A.5 reports result from the main regression for employment in targeted regulated occupations, after applying sample restrictions to obtain a balanced panel of individuals. The balanced panels include only individuals who are present in the data for each quarter-year throughout the time window. For example, in the balanced panel 2007-2017, we include only immigrants who were in the dataset in 2007 and remained through all quarters up to 2017. The other sample restrictions and the controls and FE are the same as in the main regression. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies.

Table A.6: Estimated coefficients for the effect of national and state recognition laws.

	(1)	(2)	(3)	(4)	(5)	(6)
	All regulated		Nationally regulated		State regulated	
PostNationalLaw*Non-EU	0.017 *** [0.002]	0.015 *** [0.002]	0.009 *** [0.002]	0.008 *** [0.002]		
PostStateLaw*Non-EU					0.008 *** [0.002]	0.007 *** [0.002]
Baseline (Non-EU)	6.76	6.76	4.25	4.25	2.51	2.51
R-squared	0.002	0.052	0.002	0.047	0.002	0.030
Individuals	76499	76499	76499	76499	76499	76499
Observations	1898060	1898060	1898060	1898060	1898060	1898060
Individual Controls	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes
LLM FE	No	Yes	No	Yes	No	Yes
Quarter*Region FE	No	No	No	No	Yes	Yes

Notes: Table A.6 reports result from the main regression for employment in regulated occupations with many applications (column 1 and 2), and distinguishing between occupations regulated at the national level (column 3 and 4) and at the state (*Land*) level (column 5 and 6). For the estimation of the effects of state laws the post-reform dummy is constructed based on the precise date of introduction of state laws in each region (see Figure A.5). To estimate the effect of state-level laws we include quarter-state fixed effects. Only immigrants arrived before the reform are included. The other sample restrictions and the controls and FE are the same as in the main regression. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies.

Table A.7: Effects of Recognition Act on employment in non-regulated *Mangelberufe*

	(1)	(2)
Post*Non-EU	0.0018 [0.002]	0.0024 [0.002]
Baseline (Non-EU)	6.53	6.53
R-squared	0.01	0.16
Individuals	76499	76499
Observations	1898060	1898060
Individual controls	No	Yes
Year FE	No	Yes
LLM FE	No	Yes

Notes: Table A.7 reports the estimated coefficients for regressions where the dependent variable is the probability of being employed in non-regulated *Mangelberufe*. Column 1 includes only the interaction term while Column 2 reports result from the full specification with individual controls and group fixed effects. Only immigrants who entered Germany before the reform are included, only EU15 and non-EU immigrants, aged 25-55 and with either vocational or university degree. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies.

Table A.8: Alternative sample definitions for the baseline estimation on the effect of the Recognition Act on non-EU employment in regulated occupations

	(1)	(2)	(3)	(4)	(5)
	Baseline	Education (mode)	Education (highest)	Nationality (first)	Nationality (first, no German)
Post*Non-EU	0.015 *** [0.002]	0.015 *** [0.002]	0.016 *** [0.002]	0.013 *** [0.002]	0.013 *** [0.002]
Baseline (Non-EU)	6.76	8.30	7.12	7.69	7.07
R-squared	0,052	0.046	0.050	0.057	0.054
Individuals	76499	80823	94021	67857	77257
Observations	1898060	2055295	2596231	1672058	1931648
Individual controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes

Notes. Table A.8 reports the estimated coefficients for our main regression using alternative definitions of the sample. The dependent variable is the probability of being employed in regulated occupations with many applications. Column 1 reports the baseline results from Table 3, column 4. In the baseline the sample includes EU15 and non-EU immigrants who entered Germany at age 25 or older, whose education level in the first spell is vocational or higher education and whose nationality mode is non-German. In Columns 2 and 3 we change the definition of education, first with the mode value and second with the highest value obtained. In Columns 4 and 5 we change the nationality variable first taking the first nationality and second taking the first non-German nationality. Individual controls and group fixed effects are the same as in the baseline specification. Age at entry is constant at 25+ in all specifications. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$
Source: Integrated Employment Biographies.

Table A.9: Heterogeneous effects: regression results

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Male	Female	Vocational	Higher education	25-34	35-55
Post*Non-EU	0.009 *** [0.002]	0.019 *** [0.002]	0.007** [0.002]	0.031 *** [0.003]	0.013 *** [0.002]	0.018 *** [0.003]
Baseline (Non-EU)	4.70	9.28	5.65	9.26	6.81	6.64
R-squared	0.064	0.043	0.052	0.089	0.058	0.062
Individuals	43193	33306	46170	30329	53053	23446
Observations	1060299	837761	1198203	699857	1392952	505108
Panel B	Balkan/Turkey	Africa	Middle East	Asia	North America	Latin America
Post*Non-EU	0.0005 [0.003]	0.011 *** [0.003]	0.017 *** [0.005]	0.003 [0.003]	0.010 * [0.006]	0.030 *** [0.007]
Baseline (Non-EU)	5.67	4.42	8.26	5.13	7.56	9.95
R-squared	0.067	0.069	0.066	0.058	0.062	0.061
Individuals	40552	30757	31483	38238	30563	28837
Observations	1004894	721803	734744	859328	691170	655902
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LLM FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table A.9 reports the regression results for subgroups of immigrants as specified in the column headers. The dependent variable is the probability of being employed in regulated occupations that received many applications. In **Panel A** subgroups are created according to individual characteristics. Columns 5 and 6 refer to the age of first appearance in the register data. In **Panel B** we include all EU15 immigrants and the non-EU immigrants belonging to the macroregion of origin specified in the first row of each column. Controls include sex, age, age squared, age at entry, age at entry squared, time in the registered (and its squared transformation), nationality and educational level, year fixed effects and local labor market fixed effect. Standard errors are clustered at the individual level. Significance levels: *** $p < 0.01$ ** $p < 0.05$ * $p < 0.10$

Source: Integrated Employment Biographies (IEB).

A.2 Description of Datasets and Variables

A.2.1 IEB-SOEP Migration Sample

In this section we validate the recognition variable used for the estimation of the effects of the reform on recognition rates. Given that information on recognition procedures is asked retrospectively and might therefore be subject to measurement error, we exploit other data sources on recognition procedures and compare it with the one present in the IAB-SOEP Migration Sample. In detail, we first use the 2008 ad hoc module of the German Microcensus which focused on immigrants' integration and collected information on whether immigrants applied for recognition and on the outcome of the recognition procedures. We compute the percentage of immigrants in Germany before 2012 (i.e. before the Recognition Act) with recognition, with a failed or on-going recognition procedures and with no application for recognition. We also distinguish between different types of certifications (Figure A.10). Reassuringly, we find that the distributions in the two data sets are remarkably similar. Second, we gather information from official recognition statistics on the number of applications by macroregions of origin and aggregate SOEP immigrants according to the same macroregions of origin. Since official statistics refer only to recognition procedures after 2012, we consider SOEP immigrants who applied for recognition from 2012 onwards. We then compare the composition of applicants by macroregions of origin (Figure A.11). Also in this case, the distributions are closely comparable between the two data sources. Overall, these tabs/ch2/ show that individual data on recognition from the SOEP are representative of recognition procedures.

A.2.2 Integrated Employment Biographies

It is well known that some information collected through administrative sources is less reliable because employers have low incentives to correctly declare it. In particular, in the Integrated Employment Biographies both the nationality variable and the education variable may be problematic due to misreporting or underreporting behaviors of employers. Given the relevance of these two pieces of information for our sample selection and estimation, we explain below how we improved on the raw

Table A.10: Validation of application variable: by education

	(1)	(2)	(3)	(4)	(5)	(6)
	IAB-SOEP Migration			Mikrozensus		
	Recog	No recog	No app	Recog	No recog	No app
VET	17.5	9.9	72.6	14.1	8.6	77.3
Fachhochschule	34.5	12.3	53.2	36.6	10.0	64.4
University	30.3	9.1	60.6	27.4	8.2	64.4
PhD	47.8	8.7	43.5	40.0	-	60.0

Notes: Table A.10 shows the distribution of immigrants that obtained recognition (*Recog.*), applied but did not obtain recognition (*No Recog.*) and did not apply (*No app.*) within the same type of certification. Columns 1,2,3 report the shares for immigrants in the IAB-SOEP Migration Sample who arrived in Germany in the pre-reform period, while columns 4,5,6 display the percentages for immigrants in the German Microcensus 2008 Ad Hoc Module on immigrants' integration. Source: IAB-SOEP Migration Sample and German Microcensus 2008 Ad Hoc Module.

Table A.11: Validation of application variable: by nationality

	(1)	(2)
	IAB-SOEP Migration	Register data (BIBB)
European Continent	77.8	81.0
Africa	4.0	5.5
Middle East and Asia	16.0	12.3
North and Central America	0.9	0.8
South America	1.3	1.3
Oceania and others	0.0	0.2
Total	225	17550

Notes: Table A.11 shows the distribution of applicants across macroregions of origin. In column 1 we report the shares for immigrants interviewed in the IAB-SOEP Migration Sample. In Column 2 we report the shares from the official statistics of the BIBB which were acquired from 2012 onwards to monitor recognition processes after the implementation of the Federal Recognition Act. The macroregions of origin were pre-defined in the official statistics. To match the official statistics, in the SOEP computations we include all applicants who applied from 2012 onwards and recode countries of origin to the same macroregions in the BIBB data.

Source: IAB-SOEP Migration Sample and Official Statistics (BIBB).

information and provide validating evidence on the quality of our variables.

Nationality We construct the nationality variable by taking the mode of the nationality value across all spells in the dataset. The value we assign to each individual is therefore the most frequent nationality their employers report. We then exclude all immigrants whose mode value of nationality is German and all who have no valid nationality values. While this might exclude immigrants who received citizenship early on in their employment careers, it allows to better identify the most likely foreign nationality. In alternative specifications we try also alternative definitions of nationality, that is based on the first valid nationality value and by including only immigrants who never had a spell as German natives. Results are not sensitive to this definition. Moreover, we show that the distribution across macro-regions of origin in the IEB data is almost identical to the distribution of origin countries constructed from the German Microcensus where we are able to identify more clearly both the time of migration and the foreign nationality (in the German Microcensus it is asked explicitly whether they have German citizenship).

Education Two issues with the education variable may be relevant for our analysis. First, which is the true educational level of immigrants, and second whether they acquired education domestically (i.e. in Germany) or abroad. We address both issues by using the first available information on education and by restricting our analysis to immigrants who appear in the data after 25. We choose 25 as the cut-off age of entry as we assume that by 25 immigrants already plausibly acquired both a university degree or a vocational training. Moreover, in Germany many university students and vocational trainees enter the labor market already before the end of their educational career. The restriction based on the age of entry therefore allows us to reduce the concern that education might have been acquired in Germany (and that recognition wouldn't be necessary). We then compare our education variable with the German Microcensus data where it is possible to precisely identify immigrants who acquired education abroad (from 2012 onwards the question is asked explicitly). Looking at Table 2 we see that the IEB educational variable likely underestimates the true number of university graduates (or above) both for EU15 and nonEU immigrants.

To address this issue we show in Table A.8 that results are not sensitive to changes in the definition of the educational variable. In particular, we run the main regression model using the highest level of education achieved instead of the first reported value. This includes immigrants for which employers might have falsely reported the level of education. Moreover, in case the bias from the measurement error is large, this would likely underestimate the positive effects on employment.

Occupational code Throughout the analysis we classify occupations using the 3-digits Kldb1988. For all employees, the employer encodes the employee’s job in accordance with the “Classification of Occupations. Systematic and Alphabetical Directory of Job Titles” (published by the Federal Employment Agency, Nuremberg, 1988), which contains approx. 25,000 job titles. The occupational classification Kldb1988 consists of a 3-digit code and comprises about 330 values. In December 2010 the Federal Employment Agency introduced a new classification, Kldb2010, with 5-digits. This change brought a large number of firms to misreport or underreport the occupational variable in 2011. We fix this coding problem with the following approach. We exploit other pieces of information which were not subject to any reporting change from 2010 onwards, that is work and home location (at the *Kreis* level ², industry code (WZ08 classification) and firm identification number. We then considered the last available occupational code before the reporting change and assigned this value to all subsequent employment spells, as long as work or home location, firm ID and industry code did not change. This procedure addresses both mireporting and underreporting errors. As an outcome of this procedure, missing values on the occupational code in 2011 starkly decline. With the fixed occupational code, we then move from the Kldb2010 to the Kldb1988 using a table provided by the Federal Employment Agency. This is particularly relevant to identify occupations with high and low numbers of applications since the statistics from the Regional Statistical Offices on the recognition procedures use the Kldb2010. It should be noticed that the Kldb1988 is a 3-digit classification and it is therefore more aggregated than the Kldb1988. As a robustness check, we also run regressions (available upon request) in which we exclude all individuals (any spell) if at some

²The *Kreis* level corresponds to the NUTS3 level of the NUTS geocode standard.

point of their employment history they were employed but the occupational variable had missing or invalid values. Results barely change.

Local demand for regulated occupations We construct pre-reform demand for regulated occupations in local labor markets in the following way. We obtain from the Federal Employment Agency vacancy and unemployment totals by year, occupational code (3-digit Kldb1988) and *Kreis*. Unemployment data report the last occupation of employment need to verify. The vacancy data report the numbers of positions open in each occupation as declared by firms. The unemployment to vacancy ratio captures therefore the extent to which firms are able to fill in their vacancies with local supply. We compute the unemployment-to-vacancy ratio in all Kreise and broad group of occupations (regulated occupations with large number of applicants) averaging the values for the years 2007-2010, the pre-reform period. We exclude 2011 due to its proximity to the reform. We then average the values across Kreis belonging to the same local labor market and assign to each individual the value according to its local labor market variable.

Ethnic network in regulated occupations We use the IEB data in the pre-reform period (2007-2010) to compute the average share of immigrants in regulated occupations with high number of applicants at the Kreis level. We then average the values across Kreis belonging to the same local labor market and assign to each individual the value according to its local labor market variable.

B Appendix: Chapter 3

B.1 Additional Figures and Tables

Figure B.1: Average income tax rates including employee compulsory social security contributions, with and without incentives of the 2010 tax scheme

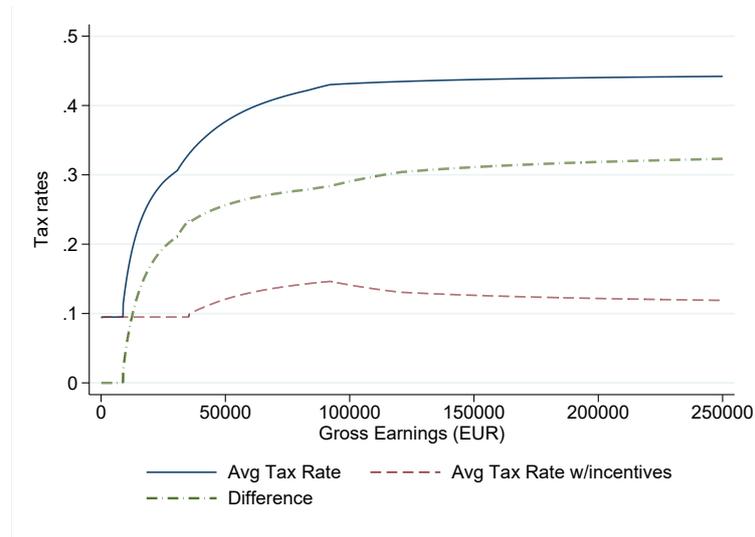
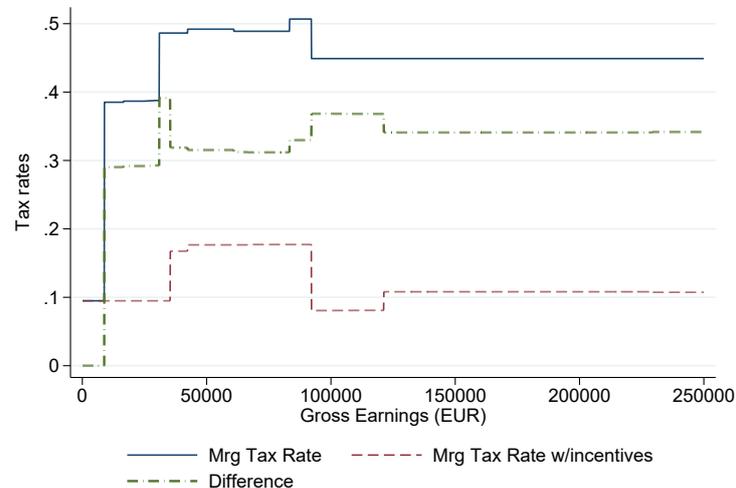
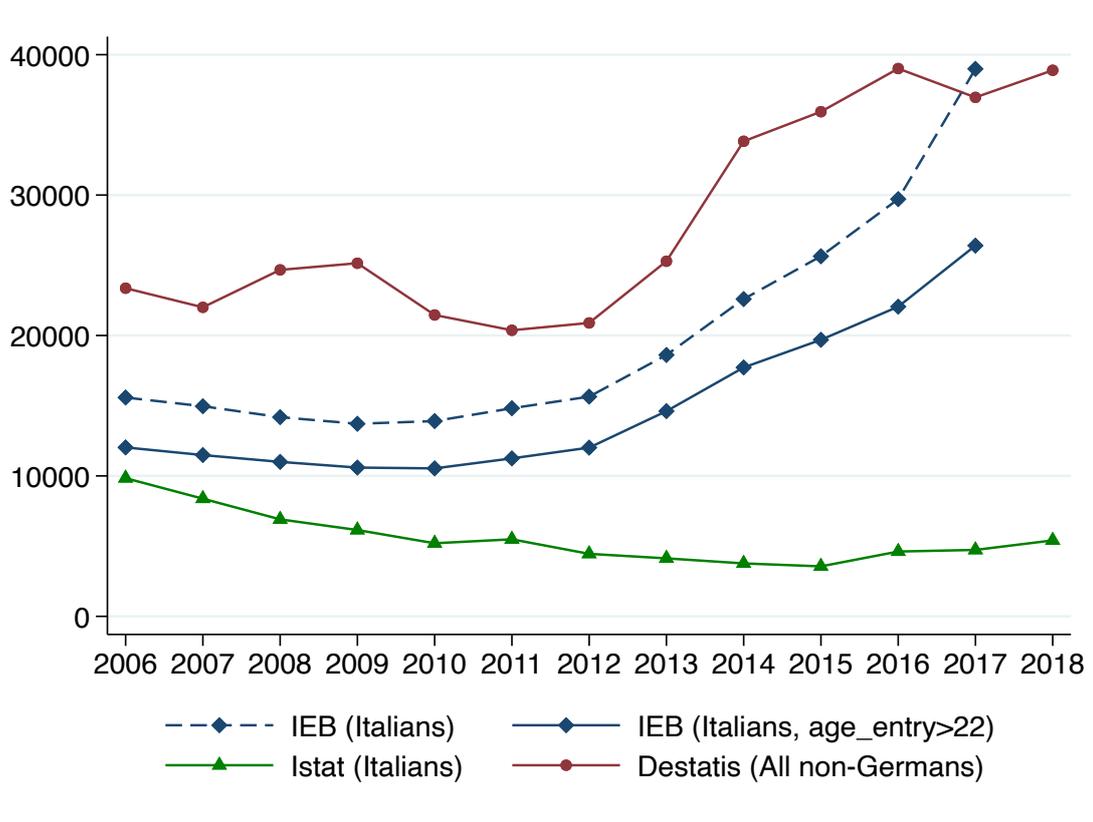


Figure B.2: Marginal income tax rates including employee compulsory social security contributions, with and without incentives of the 2010 tax scheme



Notes: the two figures plot the average and the marginal income tax rates (respectively), including employee compulsory social security contributions, based on the 2010 Italian tax schedule for an individual with no dependents (source: OECD Taxing Wages 2010). The fiscal incentive used is a 75% reduction in taxable income (Law 238/2010), i.e. an average between 70% (men) and 80% (women). For the tax rates with the tax incentives, gross earnings are assumed to be entirely from employee labor income, self-employed labor income and/or business income.

Figure B.3: Annual migration flows from Germany to Italy, by data source

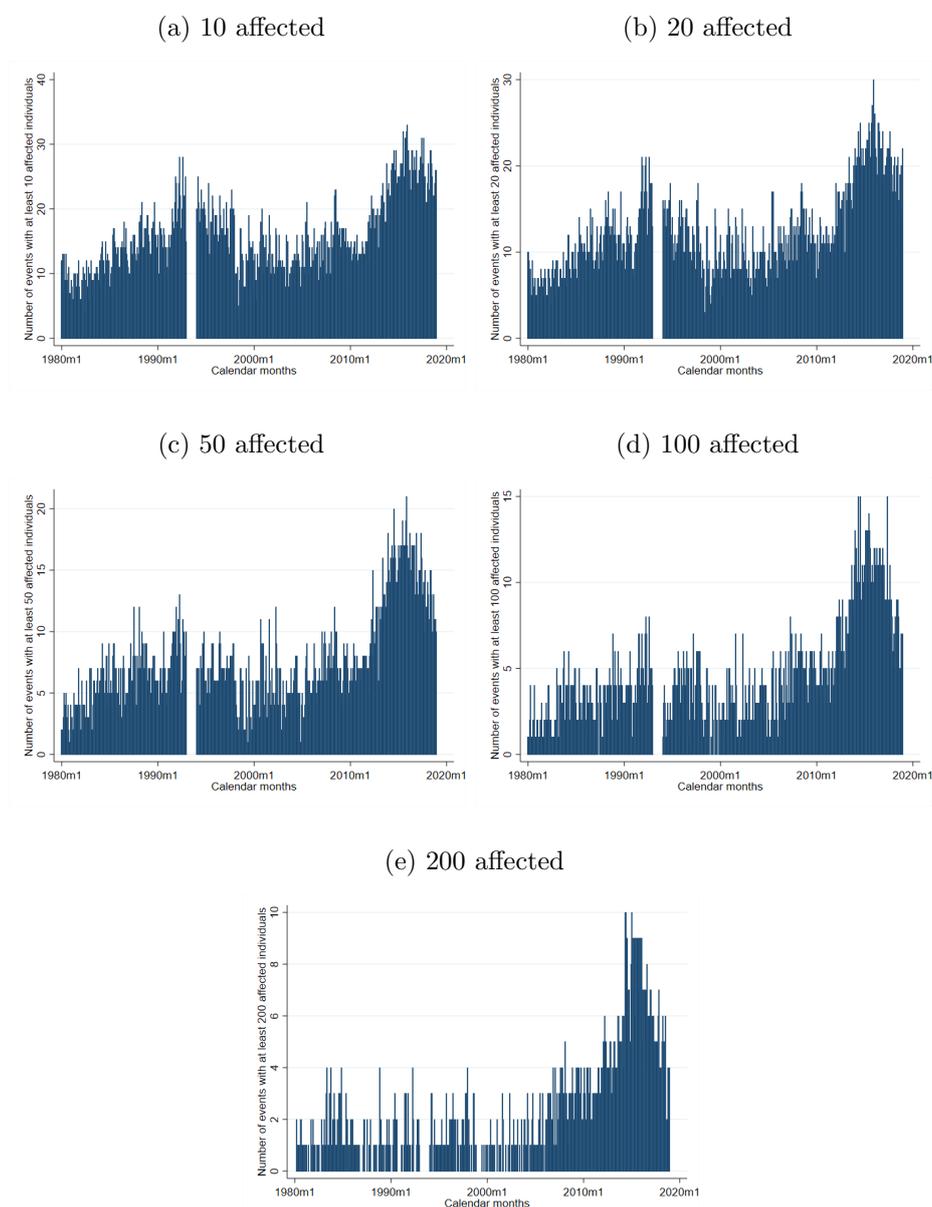


Notes: The green-triangles lines “Istat” plots the number of Italian citizens moving from Germany to Italy in each year, as recorded by Istat (the Italian statistical institute). The red-circles line “Destatis” plots the number of non-German citizens moving from Germany to Italy in each year, as recorded by Destatis (the German statistical institute). The blue-diamonds line “IEB” plots the number of Italian citizens (solid line) and the number of Italian citizens who appeared in the German social security registry after the age of 22 (dotted line) leaving the registry in each year, as recorded in the IEB data.

C Appendix: Chapter 4

C.1 Additional Figures and Tables

Figure C.1: Monthly number of events by size of the affected population



Notes: Figure C.1 displays the number of monthly terror events for different sizes of the affected population. All countries are pulled together. The y axis displays the number of events per month. The time window is 1970-2017. Source: Global Terrorism Database (GTD).

Table C.1: Balance in covariates for the GSOEP sample: joint significance

	b	SE	p-value
Female	-0.00179	0.0021	0.449
Age	0.00001	0.0001	0.952
Years since migration	0.00002	0.0002	0.894
Married	0.00003	0.0028	0.993
Education (ref: below secondary)			
Secondary education	0.00147	0.0028	0.601
Post-secondary	0.00225	0.0045	0.620
Higher education	-0.00336	0.0044	0.445
Employment (ref: full-time employment)			
Part-time employment	0.00190	0.0047	0.684
Other employment	0.00111	0.0054	0.838
Not employment	-0.00227	0.0029	0.441
Children (ref: no child)			
One child	-0.00310	0.0033	0.346
Two or more children	-0.00262	0.0043	0.549
State (ref: Schleswig-Holstein)			
Hamburg	0.00905	0.0114	0.429
Lower Saxony	0.00587	0.0089	0.510
Bremen	-0.02338	0.0160	0.144
North-Rein Westfalia	0.00347	0.0080	0.667
Hessen	0.00854	0.0087	0.328
Rheiland-Palatinate	0.00169	0.0089	0.850
Baden-Wuerttemberg	0.00288	0.0082	0.725
Bavaria	-0.00422	0.0084	0.615
Saarland	0.02148	0.0169	0.205
Berlin	0.01253	0.0102	0.221
Brandenburg	0.00923	0.0154	0.549
Mecklenburg-Vorpommern	0.00182	0.0239	0.939
Saxony	0.03303 *	0.0196	0.092
Saxony-Anhalt	0.03946 *	0.0207	0.056
Thuringia	-0.00852	0.0169	0.614

Notes: Table C.1 reports the estimated coefficients, the robust standard error and the p-value from a regression of the terror indicator on the full set of individual characteristics. The regression includes country of origin x surge year fixed effects and interview month fixed effects. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: GSOEP

Table C.2: Balance in covariates for the displaced sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Number of affected individuals at unemployment registration					
	0	10	20	50	100	200
	Mean	Diff	Diff	Diff	Diff	Diff
Female	0.28	-0.00068 [00.002]	-0.0012 [00.003]	0.00084 [00.002]	-0.00026 [00.002]	0.000095 [00.002]
Vocational education	0.67	0.0035 [00.004]	0.0022 [00.003]	0.0019 [00.003]	0.0015 [00.002]	-0.000040 [00.002]
Higher education	0.10	-0.0042 [00.005]	-0.0050 [00.005]	-0.0029 [00.004]	-0.00031 [00.004]	-0.0048 [00.003]
Age	36.4	-0.00022 [00.0001]	-0.00013 [00.0002]	-0.00023 [00.0002]	-0.00027 [00.0001]	-0.00024 [00.0009]
Experience	11.6	0.0011 [00.0002]	0.00096 [00.0002]	0.0009 [00.0002]	0.00082 [00.0002]	0.00056 [00.001]
Employed	0.02	-0.0044 [00.009]	-0.0031 [00.009]	-0.0045 [00.008]	0.00077 [00.007]	-0.0025 [00.006]
Earnings (zero included)	0.70	0.00018 [00.0003]	0.00016 [00.0003]	0.000048 [00.0003]	0.000011 [00.0002]	-0.000031 [00.0002]
UI (zero included)	13.4	-0.00015 [00.00008]	-0.00012 [00.00008]	-0.000041 [00.00008]	0.000012 [00.00007]	00.00000 [00.00005]
Re-training measures	0.05	-0.0021 [00.006]	0.0027 [00.006]	0.0020 [00.005]	0.0027 [00.004]	0.0065 [00.003]
Observations	32562	41553	39463	37098	35952	34990
Month-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Country of origin FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table C.2 reports the estimated coefficients and the robust standard error in parenthesis from regression of the terror indicator on the full set of individual characteristics. Column 1 reports the mean values for the control group (immigrants who are registered in unemployment when no event in their home countries happens). In Columns 2 to 6 the definition of treatment changes based on the number of affected individuals from terror events. All regressions include country of origin FE, survey year FE, month FE. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: Integrated Employment Biographies (IEB)

C.2 Immigrants in Germany

The current immigrant population in Germany essentially reflect three large immigration waves. The first wave started in the mid-1950s when, as a result of strong economic growth in (West-) Germany and a lack of available manpower, Germany started to actively recruit foreign workers abroad, predominantly in Turkey, Yugoslavia, Italy, Greece and Spain. Following the recession in 1973/1974, this active recruitment of immigrants was abandoned. However, subsequent immigration of family members continued. The second and more recent immigration wave to Germany was triggered by the collapse of the former Soviet Union and the political changes in Eastern Europe in the late 1980s and early 1990s. The main immigrant groups of this period were, on the one hand, ethnic German immigrants (so called *Aussiedler*), mostly from Poland and the former Soviet Union, and, on the other hand, refugees from the wars in former Yugoslavia. The third wave was in 2015-2016, when a new wave of asylum seekers arrived to Germany driven by the wars in Syria, Iraq and Afghanistan.

In Table C.3 we show the fifteen largest immigrant groups in the GSOEP survey (1984-2018) used in this study. We can see that the share of migrants in the sample accompanies well the different migration waves³

C.2.1 Migrants in the GSOEP

Figure C.2 shows the share of migrants in the GSOEP sample. When the survey started, in 1984, migrants represented about 27 percent of the GSOEP sample. At this time the main groups of foreigners were individuals from Turkey, Greece, Yugoslavia, Spain, or Italy (sample B). The share of migrants fell until 1994, when a boost sample (D1 and D2) of migrants who came to Germany after 1984 was added to take into account the flow of ethnic Germans from the former Soviet countries. After the boost sample was added in 1994-95 the share of migrants in the GSOEP fell steadily. To improve the representation of migrants living in Germany two new samples (M1 and M2) were established in 2013 which covered individuals who immigrated to Germany after 1995 or second-generation immigrants⁴. Following, the

³We discuss the migration samples within the GSOEP in appendix C.2.1

⁴Sample M1 was added in 2013 with around 2,700 households and it includes individuals who immigrated to Germany after 1995 or second-generation immigrants. Sample M2 was added in 2015

Table C.3: Largest migrant groups in the GSOEP data in %

	1985- 1990	1991- 2000	2001- 2010	2011- 2018	Total 1985-2018
Turkey	35.403	30.998	20.914	6.938	18.567
Italy	17.915	13.207	7.760	3.170	8.140
Greece	13.330	8.643	3.931	1.872	5.206
Spain	10.244	5.019	1.873	0.961	3.211
Ex-Yugoslavia	9.171	4.114	1.785	0.191	2.518
Croatia	4.601	5.105	3.029	1.018	2.751
Bosnia-Herzegovina	3.039	4.170	2.790	1.173	2.373
Poland	0.715	7.440	11.052	8.400	7.746
Kosovo-Albania	0.389	0.920	1.414	2.632	1.729
Romania	0.373	2.568	4.653	5.221	3.918
Russia	0.039	3.952	9.008	9.580	7.048
Kazakhstan	0.000	3.781	8.628	8.095	6.255
Syria	0.047	0.040	0.054	14.631	6.645
Iraq	0.000	0.020	0.171	4.575	2.110
Afghanistan	0.000	0.020	0.078	3.668	1.680

Notes: Table C.3 reports the distribution of the largest nationalities in the GSOEP over time. Shares are computed across the sample of respondents in each decade. The last column reports the distribution of the largest nationality groups in the full sample.

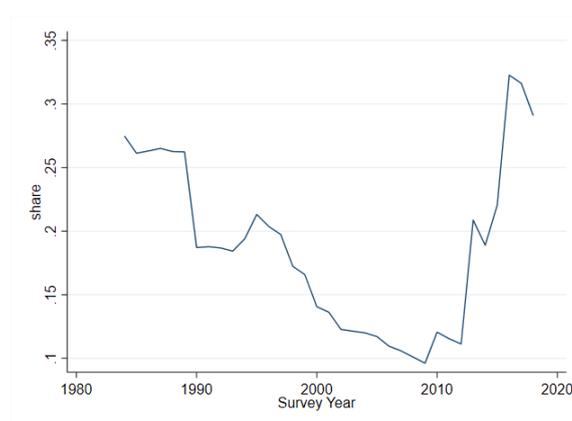
Source: GSOEP

Arab Spring and the war in Syria, a new refugee sample was added in 2016 (M3 and M4) with a subsequent booster in 2017 (M5). These samples covered households with individuals who arrived in Germany between January 2013 and December 2016 and had applied for asylum by June 2016 or were hosted as part of specific programs of the federal states⁵.

with around 1,100 households and it includes individuals who immigrated to Germany between 2010 and 2013. The samples were drawn using register information from the German Federal Employment Agency and were the product of a cooperation between the Institute for Employment Research (IAB) in Nuremberg and the German Socio-Economic Panel (SOEP) at DIW Berlin. The first seven survey waves were carried out between 2013 and 2018.

⁵The refugee samples are a joint project of the Institute for Employment Research (IAB), the Research Center of the Federal Office for Migration and Refugees (BAMF-FZ) and the Socio-Economic Panel (SOEP).

Figure C.2: Share of migrants in the GSOEP



Notes: Figure C.2 displays the share of immigrants in the sample of SOEP respondents in each survey wave. The y axis refers to the share. The time window is 1984-2019. Source: GSOEP.

